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ARTICLE I.

Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, *of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M.R.I.A. Correſponding Member of the Royal Society of Edinburgh, &c.**

IT is generally ſuppoſed that the ſeaſons in our iſland have ſuffered a conſiderable change, almoſt within the memory of the preſent generation. The winters of our climate are ſaid to have laid aſide their ancient horrors, and frequently to have aſſumed the mildneſs and vegetative powers of ſpring; while ſummer is repreſented as leſs favourable than heretofore, leſs genial in promoting vegetation, and leſs vigorous in forwarding the fruits of the earth to maturity.

It is indeed true, that in this inſtance popular opinion does not ſtand ſupported by the concurrent teſtimony of meteorological obſervations: there is no clear evidence derivable from them, that the preſent ſeaſons are materially different from former ones; and therefore philoſophers and meteorologiſts naturally aſcribe to the querulous diſpoſition of the farmer, the chill ſenſations of old age, or the predilection which every one feels for the cheerful days of childhood, the adoption of an opinion that ſeems ſo eaſily to flow from theſe ſources.

But let it be remembered, that the inſtruments of atmoſpherical obſervations do not extend to all the circumſtances which influence the crops of the farmer, or the ſenſations of the man. The thermometer may mark the general temperature of our climate as unchangeable; and the pluviometer may aſcertain its uſual moiſture; whilſt a clouded atmoſphere or a tempeſtuous wind ſhall mar the progreſſive maturity of harveſt, and ſhatter the languid frame of declining age.

* Iriſh Acad. VI.

Heat and cold and rains are, indeed, principals in the economy of ſeaſons; but winds, clouds, vapours, and other circumſtances rarely regiſtered, often unperceived, are to be deemed at leaſt ancillary in the extenſive ſyſtem; and may give plauſibility to popular ſenſations and opinions, even without the aid of meteorological teſtimony.

It is the purpoſe of this paper to offer to the Academy ſome obſervations relating to this intereſting ſubject; and to mark a few prominent events in the phenomena of our climate, which may add credibility to general report.

Of the Winds, and their Effects.

THE winds which moſt uſually prevail in our latitudes blow from the weſtward, for reaſons unneceſſary to be detailed here. Theſe winds are commonly mild in their temperature, and moiſt in their nature. They are from theſe properties extremely friendly to animal and vegetable life; and to them the great population of Ireland, and the uncommon fertility of its ſoil, may among natural cauſes be aſcribed.

But from whatever circumſtances it has ariſen, theſe winds have of late years ſwept with uncommon violence over the ſurface of our iſland; fruſtrating the uſual effects of their genial properties by the overbearing fury of their courſe; and, like Saturn, ſometimes devouring the offspring to which themſelves had given birth.

Why theſe weſterly winds have ceaſed to bear the character of zephyrs may admit of much curious and intereſting inveſtigation: at preſent I ſhall be ſatisfied with endeavouring to eſtabliſh the fact itſelf, by ſuggeſting to the Academy ſome circumſtances that ſeem to determine the matter with a very great degree of probability.

The effects of theſe winds are marked in viſible characters over the whole ſurface of the kingdom; but they are peculiarly diſtinguiſhable in the northern province of Ulſter; and chiefly in the extreme countries of that province, where a northerly latitude, joined to an expoſed ſituation on the coaſts of the ocean, forms an apt ſtation for obſervations, and exhibits as it were on a magnified ſcale the degrees of the phenomena themſelves.

Three natural regiſters of theſe effects have come within my obſervation; the trees of the country, the ſands of the ſea-coaſt, and the tides of the ocean: of each of theſe I ſhall make mention in its order.

Of the Trees.

IT is a fact extremely well eſtabliſhed, that the pine-tree, peculiarly that ſpecies vulgarly denominated the Scotch fir, formerly grew on many of the mountains of this kingdom, and on parts of the northern and weſtern coaſts, exceedingly bare and open to ſtorms. Vaſt roots and noble trunks of this ſpecies of pine have been ſeen and examined by me with attention, in ſituations where human induſtry cannot now rear a twig of the hardieſt tree. On the higheſt lands of the general ſurface of the kingdom, in the county of Weſtmeath, amid the mountains of the county of Antrim, and on the naked coaſts of Eniſhowen and Roſſes, in the county of Donegal, pine-trees have formerly arrived at an age of an hundred and twenty years, have grown to the ſize of a yard in diameter, and ſurpaſſed fifty feet in height.

There is great reaſon to think that two centuries have hardly elapſed ſince many of theſe trees grew in thoſe ſituations; and probable reaſons might be adduced to limit the great period

riod of their deſtruction to the age of James the firſt of England. In thoſe reigns, rewards were held out for ſettling the kingdom, and clearing its ſurface of foreſts, which under favour of inceſſant wars and neglected tillage, during a period of eight centuries, had overſpread the face of the country*.

The harſh and furrowed bark of this pine has occurred to me in ſuch a perfect ſtate of preſervation as almoſt alone to determine its ſpecies†.

The cones have been found by me at a depth of many feet from the ſurface of the earth, in ſuch condition as almoſt to give hope of raiſing plants from their ſeed‡. Marks of the woodman's hatchet on their trunks; veſtiges of fire applied for their deſtruction; and pieces of charcoal into which many of them have been burnt§; paleings and ſmall encloſures found at the level where they have formerly grown‖.

Leathern ſhoes, wooden veſſels filled with butter and other light ſubſtances found at conſiderable depths in turf bogs**, and not likely to have deſcended through the matted texture of that ſubſtance, give additional teſtimony to the opinion that the exiſtence of theſe bogs, and of courſe that of the trees which they contain, is not of an extremely ancient date.

It is needleſs to recall the attention of the Academy to the difficulty of raiſing trees, at preſent, in many of thoſe ſituations where the ancient pine and oak of Ireland have within the period of human exiſtence flouriſhed with luxuriance.

The labours of the farmer, the reſources of wealth and information, the rewards of patriotic ſocieties, and even the liberal encouragement of the legiſlature itſelf, have in vain ſtruggled againſt the weſtern ſtorms during the latter part of the preſent century; and the planters of our age, wearied with combating the tempeſt, have generally found it neceſſary to fly from all elevated and expoſed ſituations, and to abandon the pleaſing idea of covering the nakedneſs of mountains, the ſterility of rocks, and the bleak uniformity of bogs, with the luxuriant foliage of the oak and the pine.

Of all the foreſt trees which in later times have been cultivated for general uſe, there is none higher in the eſtimation of our farmers than the aſh. It is a tree which buds late, but finally iſſues forth ſtrong and ſucculent ſhoots: ſecure by its deciduous nature from wintry blaſts, it is nevertheleſs extremely ſenſible to the efforts of ſummer ſtorms; and becomes

* "In this reign ipe-ſtaves was one of the ordinary exports of Ireland; ſo that a mighty trade was driven with them, and thouſands of trees were felled every year for this purpoſe. A multitude of iron-mills were erected; and it is incredible how much charcoal a ſingle iron-mill will conſume in one year. So that all the great woods which the maps ſhew us, on the mountains between Dundalk and Newry, are quite vaniſhed, except one tree cloſe to the highway, at the very top of the mountain, which, as it may be ſeen a great way off, therefore ſerveth travellers for a mark."——

"Yet there are ſtill great woods remaining in Dunnagall, in Tyrone, in Antrim, &c." See Nat. Hiſt. of Ireland, by Boates, Molleneux, and others, written about the middle of the laſt century.

† In Bracknaheevlin bog, county of Weſtmeath.

‡ In Lackbeg bog, near Rutland, county of Donegal.

§ Found in a bog in the liberties of the city of Londonderry.

‖ In a bog near Surock, county of Weſtmeath; near Kilruſh, county of Clare; at Carniſk, near Ramelton, county of Donegal.

** Omitting other inſtances, two wooden veſſels containing butter were very lately found deep in a turf bog, in the Fews Mountains, near Ballymoire, the ſeat of Sir Walter Synnott. The veſſels were extremely inartificial, being little better than the hollow trunks of ſome large ſpecies of willow: the butter was inſipid, inodorous, colourleſs, ſomewhat reſembling unctuous white ſteatites in its touch and appearance; but its inflammability remained ſo perfect, as to admit of its being made into candles, to which uſe much of it was applied.

a faithful register of the winds of our climate, marking their most prevalent direction by the inclination of its boughs, and their violence, by the degree wherein its tender shoots or more mature branches are withered or blasted.

Half a century has scarce elapsed since this useful species of tree was generally planted, and grew luxuriantly in every part of Ireland: yet are there hardly any exposed places in the kingdom where its top branches do not now in one part or another exhibit the withered vestiges of commencing decay. Through many parts of Ulster it is a blasted tree; and in all unsheltered situations in the three northern counties of Antrim, Derry, and Donegal, the species seems fast verging toward annihilation*.

Attentive to each prognostic of the seasons, alive to every circumstance whereon the future subsistence of his numerous family in a populous country may depend, the farmer of the north sees these vestiges of the tempest with solicitude; and often taught of late to dread the effects of summer storms on his luxuriant crops of potatoes, he has become querulous from experience, and thinks he has some reason to complain that the seasons are less favourable to his hopes than formerly; that the pine-tree has not forsaken the mountains, nor the ash been blasted in his fields, without the influence of increasing tempests.

Of the Sands.

THE waste and dreary solitude of extended barren sands which almost every sea-coast exhibits, is generally passed over with rapidity, as useless to the philosopher from the incalculable fluctuation of its surface, and wearisome to the traveller by its disgusting uniformity.

Yet, in the midst of such a sterile scene, where nothing occurs to divert the path or distract the pensive mind of the traveller, if traces of human existence should suddenly become visible; if enclosures should appear, to mark the cheerful fire-side of some former villager, or the circuit of his little garden; if embattled walls or marble piers should start up amid the sands, suggesting ideas of ancient elegance and festivity; he must have a Stoic's mind, indeed, who will not venture to calculate causes, and feel interested in events, that come home so closely to the human heart.

On many parts of the coast of Ireland scenes such as these may be discovered.

At the entrance of the river Bannow, in the celebrated barony of Forth in the county of Wexford, vestiges of ruins, traced with difficulty amid the heaps of barren sand, serve to ascertain the site of a town, whose ancient respectability was marked by the royal charter, which endowed it with the privilege of sending representatives to the parliament of the nation, and whose opulence may be inferred from the streets which are registered in the revenue records of the last century †.

* In Lancashire, in England, between Garstang and Preston, the trees are also cut off by the westerly winds. See Newte's Tour, year 1791.

† "So late as the year 1626, Bannow is registered in the custom-house books of Wexford, as having four streets which paid quit-rent to the crown, viz. Lady-street, High-street, Weavers-street, Little-street, and some buildings surrounding the church. The only remains of Bannow which stand visible at this day (1786) are the walls of its church. There is not on or near the town but one poor solitary hut. The election for the representatives of the town is held on the walls of an old chimney adjoining to the church, which tumbled down piece-meal, and forms the council-table of that ancient and loyal corporation."

Private Letter of a Friend in the County of Wexford.

Amid

Amid the sands between Portrush and Dunluce, in the county of Antrim, in the year 1783, the ruins of a village might be seen deserted by its inhabitants, who had been obliged to move further into the country.

In the year 1787, the peninsula of Hornhead, in the county of Donegal, contained vestiges of enclosures so small and so numerous as to mark the residence of a considerable number of human families, in a spot which exhibited nothing but

——— " a desert, salt and bare,
" The haunt of seals and orcs, and sea-mews clang." MILTON.

Somewhat about a century ago, the peninsula of Rossgull, lying between the harbours of Sheep-haven and Mulroy, in the county of Donegal, was selected as the residence of one of the noble families of Hamilton, titled Boyne. It is to be presumed there was then but little apprehension, that the elegant edifice of that age should, after the short interval of an hundred years, stand, like Tadmor of the East, the solitary wonder of a surrounding desert.

For the age wherein it was built, and the style of architecture of that day, the mansion of Roseapenna may be called elegant. The approach was from a level green on the shore, through a succession of embattled courts and hanging terraces, rising in order one above the other, and adorned with marble piers of no mean design and workmanship.

The rear was ornamented with gardens laid out and planted in the fashion of the last century; and the parks and fields of the demesne seem to have been well divided and enclosed.

At present, every object in this place presents to view peculiar characters of desolation. The gardens are totally denuded of trees and shrubs, by the fury of the western winds: their walls, unable to sustain the mass of overbearing sands, have bent before the accumulated pressure, and, overthrown in numberless places, have given free passage to this restless enemy to all fertility. The courts, the flights of steps, the terraces, are all involved in equal ruin, and their limits only discoverable by tops of embattled walls visible amid hills of sand.

The mansion itself, yielding to the unconquerable fury of the tempest, approaches fast to destruction: the freighted whirlwind, howling through every avenue and crevice, bears incessantly along its drifted burthen, which has already filled the lower apartments of the building, and begins now to rise above the once elevated thresholds. Fields, fences, villages, involved in common desolation, are reduced to one undistinguishable scene of sterile uniformity; and twelve hundred acres of land are said thus to have been buried within a short period in irrecoverable ruin *.

Hence

* It would be teasing to dwell on a repetition of similar examples. I shall just mention two others: — In a summer excursion from College in the year 1787, passing from Dunfanaghy to Rutland, along the western coast of Donegal, I had great difficulty to discover a house situated between the river Guidore and the Rosses islands, whither I had been directed to enquire for a guide; and after much search at length perceived its roof just emerging from the sands. The owner told me that his house was not long built, and had at first a considerable tract of pasture ground between it and the sea-shore; but that of late he was every year obliged with great labour to dig it out of the encroaching sands, and purposed shortly to remove it to the opposite shore of a lake, called Mullochdcarg, which lay behind the house, in despair of being able to maintain his present situation.

A disputed

Hence it appears, that interesting natural events, the consequence of storms, have occurred on our coasts in the latter part of the present century, which were not foreseen, nor even suspected, at its commencement.

Of the Tides.

IT cannot be wondered at if these tempests should have had considerable effect on the tides of the ocean. The waters of that element, as long as they are unimpeded by extraneous causes, obey the influence of the heavenly luminaries, and ebb and flow with degrees of quantity and regularity of periods which come within the reach of human calculation. But, when they are agitated by storms or other violent convulsions, their quantities and periods become altogether uncertain and incalculable.

Generally speaking, agitation of the ocean, from whatever cause it may arise, produces increase in the influx and reflux of its tides, as well as deviation from their calculated times; and where the movement of this extraneous influence coincides with the natural direction of the waters, the effects are visibly distinguishable by the traces of inundation which attend their unusual progress.

As the tides of our coast raised in the Atlantic ocean flow in upon us from the westward, a storm from that quarter invariably gives them an uncommon elevation in our harbours; and this accumulation of waters sometimes anticipates the tempest itself, becoming the forerunner and prognostic of its distant commencement and approaching impetuosity.

Of late years these extraordinary influxes of the ocean have been much greater and more frequent than formerly. Every person on our coasts, whose situation has made the construction or preservation of embankments against them necessary, knows, by painful experience, how much his labours have of late years increased, and how impotent works formerly effectual are now found to be in repelling the increasing tides of the present day. Public roads encroached on; walls beaten down; strands less passable than heretofore; meadow and tillage land oftener and more deeply inundated; all concur to prove increasing tides and frequency of storms on our coasts.

These phenomena faithfully registered, extensively and distinctly delineated in natural characters, independent of every bias from human system or prejudice, free from the uncertainty of casual records or the locality of peculiar stations for observation, seem directly to demonstrate an unusual and increasing violence in the winds of our climate during the present century. That these tempests have chiefly borne upon us from the westward is plain, from the same general appearances: for, where local circumstances have not directly interfered in opposition, the trees, shrinking before the pressure from the ocean, have universally yielded to the western blast. The sands have drifted, and the tides rushed upon us from the same quarter, evidently demonstrating the more frequent recurrence and superior potency of the Atlantic storms.

[To be continued.]

A disputed boundary in Favet, on the northern coast of the same country, between the tenants of one of my own glebes and the neighbouring peasants, is ascertained by a heap of iron scoriæ in the midst of loose and shifting sands. Thirty or forty years ago, there stood here the forge of the village; but no remains of it, or its smith, are now discoverable, except the cinders of the forge, and the rank weeds that spring from a rich stratum of earth, once the soil of his garden, and still visible in a hillock of sand,

II. *Analysis*

II.

Analysis of the Red Lead of Siberia; with Experiments on the new Metal it contains. By Citizen VAUQUELIN, *Inspector of Ores*, and Conservator of Chemical Products at the Mineralogical School †.*

In arctum coacta rerum naturæ majestas. PLIN.

SECTION I.—*Historical Facts.*

THE fossil known by the name of red lead was discovered in 1770, by M. Pallas, in the gold-mine of Beresof, near Ecatherineburg in Siberia, in the form of four-sided prisms, with or without pyramidal terminations, of a beautiful orange red, commonly fixed in a quartzose matrix, to which they so strongly adhere as not to be detached without difficulty.

All the specimens of this substance which are to be found in the several mineralogical cabinets in Europe were obtained from this gold-mine; which indicates, that it was formerly abundant; but it is said, that for some years past it has become very scarce, and that at present it is bought for its weight in gold, especially if pure and regularly formed. The specimens which do not possess the regular figure, or are broken into fragments, are appropriated to painting, in which art this substance is of high value for its beautiful orange yellow colour, its unchangeableness in the air, and the facility with which it can be levigated with oil. The following are the expressions of M. Pallas, who speaks of this mineral, in his Travels in the year 1770, under the article of the Gold-mine of Pischminskoi, tom. ii. page 235:—" A very remarkable red ore of lead is likewise worked at this place, which has not yet been found in any other mine of the empire, nor elsewhere. This ore of lead is weighty, of various colours; sometimes of a cinnabar red, and semi-transparent It is fixed in long or short crystals in the clefts of quartz, and also in the bed of the mine, which is a sand-stone. It very frequently possesses, and every where when the space permits, the same thickness and prismatic form, with four plane faces and two extremities irregularly truncated. It is likewise found in small irregular contorted pyramids attached to quartz, and resembling small rubies. By reducing it to powder it affords a beautiful guhr of a deep yellow colour, which may be used in miniature painting. In all the essays of this lead ore in the laboratories of Ecatherineburg a small portion of silver has been obtained. It produces more than half its weight (valeur) in lead. Mr. Lehman could not ascertain whether this bley-spath did or did not contain silver, because his experiments were made on too small a scale to render that metal perceptible. It is difficult at present to procure the necessary quantity for trials in great, because the miners do not often work in the place where this ore is found, for want of air. In the mixed gangues of quartz in which this rare and curious mineral is formed, there are found small crystals pointed at each end, and of the colour of sulphur. They resemble native sulphur, and are considered as such by the miners; but they do not burn in the fire, nor decrepitate before the flame, like the ore of lead. This may, perhaps, be a metallic spar; but it is difficult to procure the quantity necessary

* Des mines.

† L'Ecole des Mines.—This Memoir is translated from the Journal des Mines, No. xxxiv, page 737.

cessary for examination. These small crystals are found on the sand-stone as well as on the quartz.

"I can give no other detail respecting these minerals, which will not speedily be exhausted in this country, unless the veins should extend themselves to a great depth."

SECTION II.

Account of the Experiments formerly made on the Siberian Red Lead.

THE beautiful red colour, transparency, and crystalline figure, of the Siberian red lead soon induced mineralogists and chemists to make enquiries into its nature. The place of its discovery, its specific gravity, and the lead ore which accompanies it, produced an immediate suspicion of the presence of that metal; but, as lead had never been found in possession of the characteristic properties of this Siberian ore, they thought, with justice, that it was mineralized by some other substance; and Lehman, who first subjected it to chemical analysis, asserted, in a Latin dissertation printed at Petersburgh in 1766 (I suppose 1786), that the mineralisers were arsenic and sulphur.

In 1789 Citizen Maquart undertook a long course of experiments, in which I had the advantage to participate, as he has been pleased to mention in the introduction to his work entitled *Essais de Mineralogie du Nord.* The object of these experiments was to determine the nature of the mineraliser of red lead. We sought in vain for the presence of arsenic; but, by an error, arising from the state of chemical knowledge at that time, we considered the red lead ore as a combination of super-oxygenated lead, iron and alumine.

Since that time Bindheim affirmed, that he had found it to contain molybdic acid, iron, nickel, cobalt, and copper.

From the consideration of these results, so diametrically opposite to each other, and under the encouraging consideration of the immense progress of chemical science since the renovation of its language and the rectification of its theory, and venturing likewise to place some dependance on the slight experience I have acquired in the art of essaying since I had the advantage of belonging to the establishment of mines, I thought proper to submit this substance to a new examination. My labours have not been without their recompense; and I hope to prove, in the following paragraphs, that all which has hitherto been asserted with regard to the mineraliser of the Siberian red lead is entirely destitute of foundation; that it contains neither arsenic, as Lehman pretended; nor the molybdic acid, and the three or four metals, announced by Bindheim; nor iron nor clay, as Maquart and myself imagined; but a new metal, possessing properties entirely unlike those of any other metal.

SECTION III.

The new Methods of Analysis applied to the Red Lead of Siberia.

THE science of chemistry is at present founded upon principles so certain, that, when the operator has reason to form a presumption respecting the nature of a compound body, it is possible to discover, by mere induction, the means which are most likely to separate its elements. Thus, from the suspicion that an acid might exist in the composition of the red lead, I concluded that I might decompose it in the way of double affinity; and the result of my experiments confirms the indications of theory.

The first Method.

Experiment 1.—100 parts of this mineral reduced to a fine powder were mixed with 300 parts of saturated carbonate of potash; and this mixture, together with about 4000 parts of water, was boiled for an hour. I observed that, 1. as soon as the re-action of the principles began, a strong effervescence was produced, which lasted for a long time; 2. the orange colour of the lead became a brickdust red; 3. at a certain period the whole appeared to be in solution; 4. in proportion as the effervescence proceeded, a coarse powder of a dirty yellow colour was thrown down; 5, and lastly, the fluid assumed a very fine golden yellow colour.

As soon as the effervescence had entirely ceased, and the re-action appeared to have terminated, the fluid was filtered, and the metallic powder collected. After washing and drying, it was found to weigh only 78 parts; the potash had therefore taken up 22 parts.

Experiment 2.—Upon the 78 parts last mentioned I poured nitric acid diluted with 12 parts of water. A strong effervescence followed; the greatest part of the matter was dissolved; the fluid did not acquire any colour; and the undissolved residue consisted of a small quantity of lemon-coloured powder. I separated the fluid part from the residue by means of a syphon, washed the remaining powder several times, and added the waters to the former solvent. The residue, when dried, weighed only 14 parts; whence it follows, that the nitric acid had dissolved 64 parts.

Experiment 3.—I again mixed these 14 parts with 42 parts of carbonate of potash, and the requisite quantity of water. These being treated as before, afforded the same phenomena. After filtration of the liquid, it was added to the first solution. The residue, washed and dried, weighed 2 parts. It was red lead, and was neglected.

Experiment 4.—The two nitric solutions put together and evaporated afforded 92 parts of nitrate of lead, crystallised in octahedrons, perfectly white and transparent.

These 92 parts of nitrate of lead dissolved in water were precipitated by a solution of sulphate of soda. The product was 81 parts of sulphate of lead, which answer to 56,68 of metallic lead.

Experiment 5.—The alkaline solutions, which were of an orange-yellow colour, were put together. In the course of several days they deposited two parts of a yellow powder which did not contain lead. These solutions, evaporated till a pellicle was formed on the surface, afforded yellow crystals by cooling, among which was carbonate of potash not decomposed.

These crystals were dissolved in water, and the solution, together with the mother water, was mixed with the weak nitric acid till the carbonate of potash was saturated. The fluid had then a very deep orange-red colour. When mixed with a solution of muriate of tin recently prepared, it first assumed a brown colour, which afterwards became greenish. When mixed with a solution of the nitrate of lead, there was an immediate regeneration of the red lead. And, lastly, by spontaneous evaporation, it afforded crystals of a ruby red colour, mixed with crystals of nitrate of potash.

Ninety-eight parts of this mineral, decomposed as has been here related, having afforded 81 parts of sulphate of lead, 100 parts would have afforded 82,65, which are equivalent to 57,1 of metallic lead. Now, admitting, as is proved by experiment, that 100 parts of lead absorb,

abſorb, in order to combine with acids, 12 parts of oxygen, the 57,1 of metallic lead muſt contain in the red lead 6,86 of this principle; which leaves the weight of the mineraliſing acid 36,4.

Experiment 6.—To verify by ſyntheſis the proportion of principles found by analyſis in the red lead, I diſſolved 50 grains, or about 2,654 grammes, of metallic lead in nitric acid, which was divided into two equal parts. One of theſe was completely precipitated by the requiſite quantity of the combination of the acid of red lead with potaſh; and I obtained 43 grains, or about 2,282 grammes, of red lead as fine as the native mineral.

The other portion of the nitrate of lead, precipitated by cauſtic potaſh, afforded 28 grains of the white oxide of lead. So that by this ſyntheſis 100 parts of red lead would be compoſed of 65,12 of oxide of lead, and 34,88 of acid. It gives, therefore, a difference of 1,72 leſs of acid than was deduced by the analyſis, in the mineraliſing acid of the red lead; a difference which ſhews as great a degree of accuracy as the chemical methods can afford.

SECTION IV.

The ſecond Proceſs for decompoſing the Red Lead Ore.

ANOTHER method, of which the execution is no leſs eaſy than the foregoing, conſiſts in pouring 100 parts of muriatic acid diluted with the ſame quantity of water upon 100 parts of the red lead ore in powder, and occaſionally agitating the mixture.

In this caſe the muriatic acid combines with the oxide of lead, and forms an inſoluble ſalt, which falls down, while the acid of the red lead remains diſſolved in the water which was before mixed with the muriatic acid. The fluid aſſumes a colour perfectly reſembling that of the red lead before it was pulveriſed. As ſoon as it is perceived that the muriatic acid exerts no further action on the red lead, and the precipitated muriate of lead ſtill contains ſome red particles, the ſupernatant liquor is decanted, and a new quantity of muriatic acid, one third or one fourth of the original quantity, and diluted in the ſame manner, is poured on the reſidue.

The decompoſition being complete, the ſecond ſolution is decanted, and added to the firſt; the reſidue is waſhed with a ſmall quantity of cold water, which is alſo added to the ſolutions before decanted off.

In this proceſs the operator finds himſelf under the neceſſity of obtaining his acid of red lead mixed with a certain quantity of muriatic acid, or of leaving a portion of the red lead undecompoſed.

In fact, if, according to the known proportions of the component parts, no more of the muriatic acid were to be added than is neceſſary for the ſaturation of the oxide, part of the red lead would eſcape decompoſition; for it appears that the acid of the red lead, at the time of its ſeparation, unites with a portion of the muriatic acid, which ſeems neceſſary for its more ready ſolution in the water.

Of theſe two inevitable inconveniences I have therefore preferred that which leaves a ſmall portion of the muriatic acid in the acid of red lead, becauſe it is eaſy to ſeparate it.

For this purpoſe the acid of the red lead, contaminated with muriatic acid, is to be diluted with a ſmall quantity of water, and left at reſt in a cool place for ſeveral days, in order that the ſmall portion of muriate of lead which it may contain ſhould cryſtallize and fall down. The fluid is then to be filtered or drawn off by a ſyphon; and to this muſt be added,

added, a little at a time, the oxide of silver precipitated from its solution by lime water or by a caustic alkali, and well washed.

The oxide of silver unites by preference with the muriatic acid, and forms with it a white insoluble salt, which falls to the bottom of the fluid. But great care must be taken not to add too great a quantity of the oxide of silver, because it will unite likewise with the acid of the red lead, and form an insoluble combination, which will mix with the muriate of silver. As soon, therefore, as it is observed that the oxide of silver assumes a purple red colour, it is a sign that the muriatic acid is entirely saturated, and no more of the oxide must be added. It is better, however, to add too much than too little; for, in this case, the only danger is that of losing a small quantity of the acid of red lead; whereas, in the other, the inconvenience will be, that a portion of the muriatic acid will be left in combination with the metallic acid. When the acid of red lead, mixed with muriatic acid, is evaporated to dryness, a lilac-coloured powder is obtained, which becomes green by the contact of the air, and is a combination of the oxide of the new metal with the muriatic acid.

SECTION V.

The Nature and Properties of the Acid of Red Lead.

THE acid of red lead, prepared according to the foregoing instructions, is of an orange-red colour, with a sharp metallic taste. It is very soluble in water, and its solution evaporated by a gentle heat, or spontaneously by exposure to the air, crystallizes in small long prisms of a ruby-red colour.

Experiment 1.—Paper wetted with this acid, and exposed for several days to the sun's light, assumes a green colour, which does not change in obscurity.

Experiment 2.—A plate of iron, of tin, or most of the other metals, immersed in the solution of this acid, causes it to assume the same colour.

Experiment 3.—Ether or alcohol, boiled for a few instants with this substance, produces the same effect.

Experiment 4.—The muriatic acid, heated in a retort with this acid, whether solid or in solution, produces a lively effervescence. Much oxygenated muriatic acid is afforded, and the fluid assumes a beautiful deep green colour.

Experiment 5.—These phenomena, which also take place when the ore of red lead is dissolved in the muriatic acid by means of heat, having led me to presume that the acid of red lead, by virtue of the great quantity of oxygen it contains, and the slight adherence it contracts with that principle, might favour the solution of gold in the muriatic acid, I put some leaves of that metal in a mixture of these two acids, and, by a slight ebullition, I actually obtained a complete solution of the gold.

The colour of this solution was green; it tinged the skin purple; and the solution of tin recently prepared occasioned a very abundant precipitate of the same colour.

Experiment 6.—This acid, mixed with a solution of the hydro-sulphuret of pot-ash, is precipitated in greenish brown flocks.

Experiment 7.—The aqueous solution of the tanning principle precipitates it in flocks of a yellowish brown colour.

Experiment 8.—Heated by the blow-pipe upon charcoal, it boils, and leaves a green infusible substance.

Experiment 9.—When fused with the phosphoric glass, or with borax, it communicates to the vitreous globules a very fine emerald green colour.

Experiment 10.—Lastly, this acid combines with alkalis and earths, from which it disengages the carbonic acid with effervescence, and forms, with those substances, salts more or less coloured, of which the properties are described in the following section.

From the preceding results it evidently follows, that the mineraliser of the red lead ore is a true acid; that the radical of this acid is a peculiar metallic substance; for no other metallic acid yet known exhibits properties similar to the present. In fact, What metallic acid possesses a ruby colour; communicates to all its combinations red or yellow colours, more or less deep, or yields to the muriatic acid part of its oxygen, converting it into the oxygenated muriatic acid, while itself passes to the state of a green oxide, soluble in the muriatic acid? Lastly, What metallic acid is there, which forms with mercury a combination of a cinnabar red; with silver, a carmine red compound; with lead, an orange-yellow mineral; with the hydro-sulphuret of potash, an olive-green, &c.? If I am not deceived, there is none such. Therefore, notwithstanding the repugnance which I have to admit new simple bodies, a repugnance grounded on the numberless modifications which nature may give to bodies already known, and render them apparently new, I am nevertheless forced, by the great number of new characters possessed by this and by no other substance, to regard it as a metal naturally acidified, which resembles no other we are acquainted with.

This opinion will be still more confirmed by the experiments in the following paragraphs.

SECTION VI.

Combinations of the Acid of Red Lead with Alkalis, Earths, and Metallic Oxides.

THE small quantity of native red lead which I have hitherto possessed, has not allowed me to prepare any large masses of the salts which this acid is capable of forming with the alkaline, earthy, and metallic substances, in order to examine their properties with all the precision necessary to render them perfectly known. I shall confine myself, therefore, in this place, to exhibit their principal characters, such as colour, solubility in water, taste, their habitudes in the fire, &c. reserving myself to return to this object when circumstances shall be more favourable. It is well known, likewise, that a great interval of time frequently takes place between the period when the discovery of an uncommon substance has been made, and that in which all its properties are perfectly known, and that researches of this kind are usually indebted to time and opportunity for their ultimate degree of perfection.

Combination of the Acid of Red Lead with Barytes.

THE acid of red lead ore readily unites with barytes, with which it forms a salt of very sparing solubility; for, by pouring an aqueous solution of this acid into a solution of the earth, a precipitate is formed of a pale lemon-yellow colour. This salt, however, is not entirely insoluble; for the supernatant fluid still retains a slight yellow colour, though the two principles of the salt are mutually saturated. This pulverulent salt has no perceptible

taste;

taste; it is decomposed by the mineral acids; it gives out oxygen gas by heat, and the residue is an earthy mass of a green colour.

Combination of the Acid of Red Lead with Lime.

LIME combines with the acid of red lead, and affords a salt which does not appear to be more soluble than the foregoing: for, by mixing lime-water with a solution of this acid, a deposition of an orange-yellow colour is made, which is less abundant than with barytes; a consequence which naturally follows, from the barytes being about twenty times more soluble in cold water than lime; in consequence of which the precipitate formed with this last earth must be twenty times less in quantity. A large quantity of salt, of the same colour as the former, is also obtained by evaporation of the fluid. The salt formed by the combination of lime and the acid of red lead does not appear to differ from that of barytes, except in its being less soluble, and possessing different affinities and proportions of component parts.

Its habitudes with acids, and with heat, are the same as those of the combination of barytes with the same acid.

(To be concluded in our next.)

III.

Information respecting the Earth of the Beryl; in Continuation of the first Memoir on the same Subject. By Citizen VAUQUELIN.*

I HAVE announced in my Memoir on the Earth of the Beryl, that this fossil contains about 8 per cent. of that principle; but at the same time I observed, that I did not consider this proportion as being very accurate, because I presumed that a certain quantity had been dissolved by the potash employed to separate the alumine.

I also announced that I had begun some trials, to ascertain whether the alumine contained in the beryl was in fact contaminated by a mixture of this new earth. The result of these experiments, related in a few words, will form the subject of the present communication to the Institute, together with an account of certain properties of this substance, of which I have since extracted a larger quantity.

Experiment 1. I put together the alumine which had been obtained from three docimastic quintals of beryl, which had been analysed in the same number of separate operations. The quantity was 63 grammes. I dissolved them in sulphuric acid, and, after having (brévetté) the solution, I submitted it to evaporation to obtain the alum. The evaporation was continued until crystals were no longer afforded. The remaining mother water was very saccharine and thick.

Experiment 2. I mixed this mother water with a solution of carbonate of ammoniac more than sufficient to saturate the acid. The mixture was repeatedly shaken during twenty-four hours. I perceived that the bulk of the precipitate thrown down by the first portions of the

* The first Memoir is inserted in the present volume of our Journal, page 358. This continuation is translated from the Annales de Chimie, xxvi. 170.

carbonate

carbonate had very perceptibly decreafed. At the expiration of that time I filtered the fluid, to feparate the undiffolved part: this laft, after wafhing and ignition, weighed 5 grammes, and had all the properties of alumine.

Experiment 3.—I expofed the ammoniacal folution to the action of heat in a capfule of porcelain: as foon as the temperature was fufficiently elevated to drive off the carbonate of ammoniac, a large quantity of a white, granulated, and very voluminous earth was depofited. I continued the evaporation until all the ammoniacal falt was diffipated. I then threw the refidue upon a filter, and wafhed it with much water. This refidue, after drying by a gentle heat, was perfectly white, pulverulent, and foluble in acids, with a ftrong effervefcence. It weighed 42 grammes, which by a red heat were reduced to 25 grammes; whence it follows, that thefe 42 grammes contained 17 grammes of carbonic acid and water.

Confequently, the three quintals of beryl having afforded 24 grammes of the new earth which was not diffolved by potafh, and the alumine which thefe three quintals afforded having alfo, as has been juft obferved, 25 grammes of the fame fubftance, it is evident that each quintal of beryl contains 16,33. One hundred parts of beryl are therefore compofed of

69	parts filex,
16	earth of beryl,
13	alumine,
1	oxide of iron,
0,5	lime.
99,5	

Experiment 4.—I have obferved in my firft Memoir, that alumine diffolved in the nitric acid was precipitated by the earth of beryl. I was defirous of knowing whether the fame phenomenon would likewife take place with alum. I therefore diffolved one hundred parts of this falt in about fix hundred parts of hot water. The folution afforded cryftals of alum. Into this folution I put a certain quantity of the earth of beryl, recently precipitated from a folution by ammoniac, and well wafhed. The mixture was boiled for an hour. I foon perceived that the earth of beryl was taken up; and in proportion as the excefs of acid in the alum was faturated, there fell down a great quantity of earthy matter, in white flocks, in a very divided ftate. As foon as the decompofition appeared to me to be complete, I filtered the liquor, and collected the precipitated earth, which, when wafhed and diffolved in the fulphuric acid, afforded, with a fufficient quantity of fulphate of pot-afh, cryftals of alum perfectly octahedral.

The fluid from which the alumine had been feparated poffeffed a very faccharine tafte; and, when fubjected to evaporation, it did not afford alum. Hence we find, that the earth of beryl has a greater affinity than alumine with refpect to the fulphuric acid, as well as the nitric.

Experiment 5.—Being defirous of afcertaining ftill more effectually the difference which exifts between alumine and the earth of beryl, I diffolved ten parts of the former earth in the fulphuric acid; and after having added the neceffary quantity of fulphate of potafh,

ash, I obtained, by several successive crystallizations, 90 parts of alum. I also dissolved 10 parts of the earth of beryl in the same acid, and added the same quantity of sulphate of pot-ash. I obtained only 50 parts of salt in small crystalline grains, of which I could not determine the figure. This salt is soluble in 7 or 8 parts of cold water, a quantity very insufficient to dissolve alum at the same temperature.

There cannot, therefore, remain any doubt concerning the particular nature of the earth contained in the beryl; which must henceforward be reckoned in the number of substances of this order, of which it will compose the eighth species.

It almost always happens in the sciences of observation, and even in the speculative sciences, that a body, a principle, or a property, formerly unknown, though it may often have been used, or even held in the hands, and referred to other simple species, may, when once discovered, be afterwards found in a great variety of situations, and be applied to many useful purposes.

Chemistry affords many recent examples of this truth. Klaproth had no sooner discovered the different substances with which he has enriched the science, but they were found in various other bodies; and if I may refer to my own processes, it will be seen, that after I had determined the characters of chrome, first found in the native red lead, I easily recognised it in the emerald and the ruby. The same has happened with regard to the earth of the beryl. I have likewise detected it in the emerald; in which, nevertheless, it was overlooked both by Klaproth and myself in our first analysis: so difficult it is to be aware of the presence of a new substance, particularly when it possesses some properties resembling those already known!

Though I have not yet determined with much accuracy the proportion in which this earth exists in the emerald, I think, nevertheless, that it is nearly the same as in the beryl; but I shall ascertain this point with more certainty in my second analysis. The emerald and the beryl are therefore two stones of the same nature, excepting the colouring part; and the sciences of crystallography and chemistry are here also perfectly consistent in their results.

A Table of the general Properties of the Earth of the Beryl.

1. It is white.
2. Insipid.
3. Insoluble in water.
4. Adhesive to the tongue.
5. Infusible.
6. Soluble in the fixed alkalis.
7. Insoluble in ammoniac.
8. Soluble in the carbonate of ammoniac.
9. Soluble in almost every one of the acids (except the carbonic and phosphoric acids), and forming salts of a saccharine taste.
10. Fusible with borax into a transparent glass.
11. Absorbs one-fourth of its weight of carbonic acid.
12. Decomposes the aluminous salts.
13. Is not precipitable by well-saturated hydro-sulphurets.

A Table

A Table of the specific Characters of the Earth of Beryl.

1. Its salts are saccharine, and slightly astringent.
2. It is very soluble in the sulphuric acid by excess.
3. It decomposes the aluminous salts.
4. It is soluble in the carbonate of ammoniac.
5. Is completely precipitated from its solutions by ammoniac.
6. Its affinity for the acids is intermediate between magnesia and alumine.

None of the known earths unite the six properties announced in this table.

I present to the Institute a certain quantity of this earth, and shall produce at one of its future sittings a series of combinations formed with this earth, extracted from a considerable quantity of beryl given to me by Citizen Patrin, whose zeal for the advancement of the sciences is well known to every one of their cultivators.

IV.

Observations on Electricity, Light, and Caloric, chiefly directed to the Results of Dr. PEARSON's *Experiments on Electric Discharges through Water. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

WHEN I received the 6th, 7th, and 8th Numbers of your Journal for 1797, in succession, I read Dr. Pearson's Experiments and Observations on the Gaz produced by passing Electric Discharges through Water *, with attention.

His experiments appeared to me to be well devised and conducted, and his conclusions fair and satisfactory: but his explanations of the manner in which those gazes were produced from water, and then re-converted into water by the same agent, the electric fluid, were by no means satisfactory.

That I might not be premature in my decisions against Dr. Pearson's principles or reasonings, I diverted my attention from the subject till now; but, upon the re-perusal of his papers, I still think his principles lax and indeterminate, and his reasonings, in part, unphilosophical, and to me apparently indefensible. I take the liberty, therefore, of laying the few following animadversions and objections before you:

The electric fluid, common fire, and light, those universal and general agents of nature, so far from being understood, appear to me to be not only confounded, but also to be so imperfectly considered, as to be the cause of endless confusion in every department of philosophy.

Oxygen gaz is said to be formed of oxygen and caloric:—what idea then are we to form of caloric?—Is it a simple, homogeneous principle, or is it a compound?—The general idea, or at least the common expression, seems to imply the idea of its being simply the calorific principle, and consequently uncompounded.

Oxygen may be converted into oxygen gaz by the electric fluid, or by light, as well as by fire. The electric fluid, then, imparts caloric to oxygen, and so does light.—What

* Philos. Journal I. 241, 299. 349.

ideas

ideas then are we to form of the electric fluid and of light?—Are they merely modifications of simple caloric, or are they compounds in which caloric forms a part?

If the electric fluid be a mode of existence of simple, homogeneous caloric, how are we to explain the fact *, that certain electric atmospheres repel each other, which atmospheres will attract other electric atmospheres that are also repulsive to each other?

It is incontrovertibly evident that electric atmospheres are of two distinct kinds; equally extensive, equally powerful, and mutually attractive to each other; although each is invariably repulsive to every atmosphere of the same kind as itself; and it is equally certain that they only produce fire, or take that form when they rush together, and cease to be electric. Instead, then, of supposing the electric fluid to be caloric, or partly formed of caloric, it appears to me to be more philosophical to suppose that it consists of two principles † in states of separation, which form caloric when they quit their electric states, and combine.

Is light a simple fluid ‡, or a modification of caloric?—If so, what ingenuity can explain how one homogeneous fluid, or simple fire, can, by passing through a prism, be separated into such variety of parts so permanently dissimilar to each other? and how happens it that light never produces fire but by evidently changing its mode of existence, as it ceases to be light when it takes the state and properties of fire?—This consideration again seems to lead to the conclusion that light consists of distinct and dissimilar principles, which, combining together, lose their properties as light, and constitute a fluid of very different properties and character, which we call fire, or caloric.

Nay, even if we advert to caloric itself, is it an uncompounded principle, or can any simple principle possibly perform the various parts assigned to it?—According to the present system of chemistry, caloric, homogeneous, simple caloric, destroys combinations which itself had formed; it attaches itself to particles of matter, and forms itself into repulsive spheres around them: and yet certain spheres of caloric in this state of repulsion will rapidly attract other spheres of the same caloric in similar states of repulsion! In short, caloric is hot or cold, attractive or repulsive, visible or invisible, just as occasion may serve; and, Proteus-like, it takes all shapes and forms:—we dread to meet it in Jove's thunderbolt, and court its influence in the cooling breeze!

* The facts are simply, that *bodies* attract and repel each other in certain states of electrization; but the existence of electric atmospheres, of one simple electric fluid, or of two, &c. are mere hypotheses which yet remain without proof. M. de Luc (Idées sur la Meteorologie) supposes electricity to consist of the matter of heat combined with another matter incapable of passing through glass, &c. and that the charge is produced by the condensation of this last with the transmission of heat, in the same manner as steam (water and heat) might be condensed on a pane of glass, and cause the evaporation of water, supposed to be placed on the opposite surface, by virtue of the transmitted heat. N.

† That light of every kind is emitted by the electric spark is easily seen with the prism. N.

‡ The great Leonard Euler, and others, have maintained that the sensations and effects of light are merely consequences of the vibrations of a rare and very elastic fluid. To this doctrine have been opposed the rectilinear motion of light, which does not flow into the lateral spaces, together with its reflection, refraction, and colours. Much complexity of vibration would indeed be required to account for these phenomena: but not more, perhaps, than necessarily results from the consideration of sound; its echo, which deviates little from the angle of reflection; and the harmony, melody, tone, &c. of its distinct cotemporaneous and successive impressions. N.

Having thus, in a general way, pointed out the confusion which obscures and depreciates philosophy in consequence of employing words without ideas, and of admitting certain agents as simple principles which probably are compounds, without deigning to consider whether they are so or not, even though the most palpable absurdities attend the admission; permit me to make a few critical remarks upon the paper in question.

Dr. Pearson explains the production of the gazes from water, by supposing that the dense electric fire, at the moment of diffusion, interposes betwixt the constituent elements of water, and places them beyond the sphere of attraction for each other, when each ultimate particle of the oxygen and hydrogen unites with a determinate quantity of fire, and they form hydrogen gaz and oxygen gaz.

It appears, then, that electric fire forcibly destroys the chemical union between oxygen and hydrogen; and that then a portion of it assumes the state of caloric, attaches itself to the particles of oxygen and hydrogen, and counteracts their chemical union, by keeping them distant from each other in the state of gaz.

Dr. Pearson then proceeds to explain in what manner these two gazes are made to recombine, and form water, by means of the electric fire or caloric. He first points out the manner in which he supposes the caloric acts in these words:—"I conceive its agency to be merely diminishing or destroying the powers which counteract chemical union;" and then proceeds to apply his theory to the explanation of the production of water from hydrogen gaz, and oxygen gaz, by the agency of caloric, in these words:—"Accordingly, when an electric spark, or the smallest particle of flame, or of an ignited substance, is applied to the gaz produced in the above process, or to the mixture of hydrogen and oxygen gaz, the ultimate particles of these gazes nearest to the flame are driven from it in all directions, as from a centre, by the interposition of fire, or of caloric and light; so that they are brought within the sphere of their chemical attraction for the ultimate particles of the gases at a certain distance from the centre of application of fire, which therefore unites," &c. Now, I must take the liberty to say, that I think this no explanation whatever. The conclusion appears to me to be drawn from premises which neither warrant such conclusion, nor make it either probable or conceivable.

According to Dr. Pearson's own theory and explanation, caloric counteracts the chemical union betwixt oxygen and hydrogen, and holds them in the state of gas; and, according to his own principles, he ought to have explained in what manner the caloric of an ignited body diminishes, or destroys, the spheres of caloric which counteract their chemical union; instead of which he only tells us, that caloric drives the ultimate particles of those gazes, nearest to it, in all directions, which, therefore, combine with other particles at a distance; but gives no idea whatsoever of the manner in which that flame, or that caloric, flowing off from an ignited body, diminishes or destroys the caloric which is already attached to the particles of oxygen and hydrogen, and counteracts their chemical affinity or union. In short, if caloric actually is attached to the disunited particles of oxygen and hydrogen, and prevents their chemical union, nothing that Dr. Pearson has said tends, in the least, to show how the accession of still more caloric can either diminish, or destroy, the caloric already attached to them, in sufficient quantity to prevent their union.

Much might be said upon a subject like this; but, as I wish not to take up too much of your time, I shall not proceed further with my remarks at present. What I have already advanced

advanced is, I think, sufficient to shew that Dr. Pearson's explanation of the subject in question is neither philosophical nor admissible; and, for the credit of the doctrine which he espouses, some more satisfactory explanation should be given. With respect to Dr. Parr's theory, that light and fire repel each other, when disengaged from matter, it appears to me too fanciful to be solid, and too inconsistent to be supported. If light and fire repel each other when disengaged from matter, by its decomposition, it is not an easy matter to shew how they, when in states of freedom and consequent repulsion, were brought together on the same substance at its formation: and, if oxygen and light repel each other, as he contends, why does light combine at once with the oxygen of nitric acid, and form oxygen gas, by merely placing the acid in its way? But this is not the hypothesis I have undertaken to combat.

I offer this letter for your perusal, and confess that I wish to see the subject impartially attended to. It is painful and humiliating to see how readily the most glaring absurdities are overlooked, nay, even adopted as principles, and employed as indisputable facts; and no further proof is necessary to point out the confusion and absurdity to which such erroneous principles tend, than the result of Count Rumford's experiments and reasonings on the production of heat by friction *; which is, that as heat thus produced by friction cannot be accounted for upon the adopted principles of chemistry, he seems inclinable to conclude, that it cannot be a material substance, and is most probably nothing but motion!!! Surely this is sacrificing too much to hypothesis, to give up the conviction of reason, and the testimony of the senses, rather than suppose it possible that a favourite theory may be false. The production of heat by friction, is, no doubt, inexplicable upon the principles of M. Lavoisier, which ought to excite the suspicion of those who have embraced them; for, if so simple a fact cannot be explained by those principles, it is, at least, probable, that they are not deserving of that confidence which is so generally and implicitly placed in them. The production of heat, *ad libitum*, by friction or percussion between solid bodies, does, however, admit of an easy explanation, without being driven to the necessity of making caloric a non-entity, or heat a particular kind of motion †, propagated in such a particular

* Philos. Journal, II. 106.

† The prodigious quantity of heat which follows from an apparently minute action, has always been considered as a strong argument against the hypothesis, that heat is mere motion; but, upon close examination, it is found to apply to both theories. If a very minute portion of a large mass of oxygen gas and carbon be *put into a state of vibration* (or heated), and the combination of this first portion be succeeded by vibrations and combinations of the remaining parts of the mass, we shall be led to investigate the attractive and repulsive powers concerned in the phenomena, and shall in all probability find the task by no means easy. But will our difficulties be alleviated or removed by supposing the presence of a third substance (caloric) in previous combination with the oxygen, which will certainly demand an exhibition of similar powers to account for its transition? Whatever may be the supposed play of the affinities or powers *with caloric*, it seems probable that the mere theory might be constructed as well (I do not say better) *without it*.

It has been said that latent motion (or heat) is an absurdity, as in terms indeed it is; but this might be explained in a variety of ways. Caloric is absorbed, or motion is accumulated, in certain processes. Slight incidents, not commensurate to the effect, set them free. He who sets the catch of the pile-engine at liberty, or discharges a cross-bow, or breaks the tail of Prince Rupert's drop, or communicates a spark to a powder-magazine, may be said to give effect to latent motion, or rather to destroy the equilibrium of forces generated by former efforts, far superior to that employed to set them at liberty.

Though

particular kind of manner, that the motion of a ſingle ſpark of fire, directed into a powder-magazine, will not only give motion to the building itſelf, and its contents, but will alſo ſhake a county!

Should this letter be thought unworthy of attention, or unfit for the public eye, I could wiſh, however, that you would oblige me ſo far, if convenient, as to tranſmit it to Dr. Pearſon. I have the higheſt opinion of his chemical knowledge, as well as of the liberality of his principles; and though I have plainly pointed out ſome parts of his writings which I think erroneous, I ſtill hold him no leſs in eſtimation. My only motive in making the objections and remarks which I have, is, if poſſible, to arrive at truth.

I am, with grateful eſteem for the valuable information which I have repeatedly received from your labours in the fields of ſcience, Sir,

Your moſt obedient ſervant,

Nov. 16, 1798.

A conſtant Reader of your valuable Publications.

Though I have ſtated a few facts and obſervations, I by no means wiſh to be thought a maintainer of any of the theories alluded to in the text or the notes.—None of them appear to me to be eſtabliſhed upon indubitable facts.

V.

An Inquiry concerning the Chemical Properties that have been attributed to Light. By BENJAMIN, *Count of Rumford, F. R. S. M. R. I. A.**

IN the ſecond part of my ſeventh eſſay, (on the propagation of heat in fluids,) I have mentioned the reaſons which had induced me to doubt of the exiſtence of thoſe chemical properties in light that have been attributed to it, and to conclude, that all thoſe viſible changes produced in bodies by expoſure to the action of the ſun's rays, are effected, not by any chemical combination of the matter of light with ſuch bodies, but merely by the heat which is generated, or excited, by the light that is abſorbed by them.

As the deciſion of this queſtion is a matter of great importance to the advancement of ſcience, and particularly to chemiſtry, and as the ſubject is in many reſpects curious and intereſting, it has often employed my thoughts in my leiſure hours; and I have ſpent much time in endeavouring to contrive experiments, from the unequivocal reſults of which the truth might be made to appear. Though I have not been ſo ſucceſsful in theſe inveſtigations as I could wiſh, yet I cannot help flattering myſelf, that an account of the reſults of ſome of my late experiments will be thought ſufficiently intereſting to merit the attention of the Royal Society.

Having found that gold, or ſilver, might be melted by the heat (inviſible to the ſight) which exiſts in the air, at the diſtance of more than an inch above the point of the flame of a wax-candle, (ſee my ſeventh eſſay, part II. page 350.†) I was curious to know what effect this heat would produce on the oxides of thoſe metals.

Experiment No. 1. Having evaporated to dryneſs a ſolution of fine gold in aqua regia, I diſſolved the reſiduum, in juſt as much diſtilled water as was neceſſary in order that the ſo-

* Philoſ. Tranſ. 1798, p. 449. † Or Philoſ. Journal, II. 165.

lution

lution (which was of a beautiful yellow colour) might not be diſpoſed to cryſtallize; and, wetting the middle of a piece of white taffeta riband, $1\frac{1}{2}$ inch wide, and about eight inches long, in this ſolution, I held the riband, with both my hands, ſtretched horizontally over the clear bright flame of a wax candle; the under ſide of the riband being kept at the diſtance of about $1\frac{1}{2}$ inch above the point of the flame. The reſult of this experiment was very ſtriking. That part of the riband which was directly over the point of the flame, began almoſt immediately to emit ſteam in denſe clouds; and, in about 10 ſeconds, a circular ſpot, about $\frac{3}{4}$ of an inch in diameter, having become nearly dry, a ſpot of a very fine purple colour, approaching to crimſon, ſuddenly made its appearance in the middle of it, and, ſpreading rapidly on all ſides, became, in one or two ſeconds more, nearly an inch in diameter.

By moving the riband, ſo as to bring, in their turns, all the parts of it which had been wetted with the ſolution to be expoſed to the action of the current of hot vapour that aroſe from the burning candle, all thoſe parts which had been ſo wetted, were tinged with the ſame beautiful purple colour.

This colour, which was uncommonly brilliant, paſſed quite through the riband, and I found the ſtain to be perfectly indelible. I endeavoured to waſh it out; but nothing I applied to it, and among other things I tried ſuper-oxygenated marine acid, appeared in the ſmalleſt degree to diminiſh its luſtre. The hue was not uniform, but varied from a light crimſon to a very deep purple, approaching to a reddiſh brown.

I ſearched, but in vain, for traces of revived gold, in its reguline form and colour; but, though I could not perceive that the riband was gilded, it had all the appearance of being covered with a thin coating of the moſt beautiful purple enamel, which, in the ſun, had a degree of brilliancy that was ſometimes quite dazzling.

Experiment No. 2. A piece of the riband which had been wetted with the aqueous ſolution of the oxide, was carefully dried in a dark cloſet, and was then expoſed, dry, over the flame of a burning wax candle. The part of the riband which had been wetted with the ſolution (and which on drying had acquired a faint yellow colour) was tinged of the ſame bright purple colour as was produced in the laſt-mentioned experiment, when the riband was expoſed wet to the action of the heat*.

Experiment No. 3. A piece of the riband which had been wetted with the ſolution, and dried in the dark, was now wetted with diſtilled water, and expoſed wet to the action of the aſcending current of hot vapour which aroſe from the burning candle: the purple ſtain was produced as before, which extended as far as the riband had been wetted with the ſolution, but no farther.

I afterwards varied this experiment in ſeveral ways, ſometimes uſing paper, ſometimes fine linen, and ſometimes fine cotton cloths, inſtead of the ſilk riband; but nearly the ſame tinge was produced, whatever the ſubſtance was that was made to imbibe the aqueous ſolution of the metallic oxide.

Similar experiments, and with ſimilar reſults, were likewiſe made with pieces of riband,

* We ſhall hereafter find reaſon to conclude, that the ſucceſs of this experiment, or the appearance of the purple tinge, was owing to the watery vapour which exiſted in the hot current that aſcended from the flame of the candle. R.

fine

fine linen, cotton, paper, &c. wetted in an aqueous solution of nitrate of silver; with this difference, however, that the tinge produced by this metallic oxide, instead of being of a deep purple, inclining to crimson, was of a very dark orange colour, or rather of a yellowish brown.

In order to discover whether the purple tinge, in the experiments with the oxide of gold, was occasioned by the *heat* communicated by the ascending current of hot vapour, or by the *light* of the candle, I made the following experiment, the result of which I conceive to have been decisive.

Experiment No. 4. A piece of riband was wetted with the aqueous solution of the oxide of gold, and held vertically by the side of the clear flame of a burning wax candle, at the distance of less than half an inch from the flame.

The riband was dried, but its colour was not in the smallest degree changed.

When it was held a few seconds within about $\frac{1}{8}$ of an inch of the flame, a tinge of a most beautiful crimson colour, in the form of a narrow vertical stripe, was produced.

The heat which existed at that distance from the flame, *on the side of it*, where this coloured stripe was produced, was sufficiently intense, as I found by experiment, to melt very fine silver wire, flatted, such as is used in making silver lace.

The objects I had in view in the following experiments are too evident to require any particular explanation.

Experiment No. 5. Two like pieces of riband were wetted at the same time in the solution, and suspended, while wet, in two thin phials, A and B, of very transparent and colourless glass; the mouths of the phials being left open. Both these phials were placed in a window which fronted the south; that distinguished by the letter A being exposed naked to the direct rays of a bright sun; while B was inclosed in a cylinder of pasteboard, painted black within and without, and closed with a fit cover, and consequently remained in perfect darkness.

In a very few minutes, the riband in the phial A began sensibly to change its colour, and to take a purple hue; and, at the end of five hours, it had acquired a deep crimson tint throughout.

The phial B was exposed in the window, in its dark cylindrical cover, three days; but there was not the smallest appearance of any change of colour in the silk.

Experiment No. 6. Two small parcels of *magnesia alba*, in an impalpable powder, (about half as much in each as could be made to lie on a shilling,) were placed in heaps, in two china plates, A and B, and thoroughly moistened with the before-mentioned aqueous solution of the oxide of gold. Both plates were placed in the same window; the moistened earth in the plate A being exposed naked to the sun's rays; while that in the plate B was exactly covered with a tea-cup, turned upside down, which excluded all light.

The *magnesia alba* in the plate A, which was exposed to the strong light of the sun, began almost immediately to change colour, taking a faint violet hue, which by degrees became more and more intense, and in a few hours ended in a deep purple; while that in the plate B, which was kept in the dark, retained the yellowish cast it had acquired from the solution, without the smallest appearance of change.

Experiment No. 7. A small parcel of *magnesia alba*, placed on a china plate, having been moistened

moiſtened with the aqueous ſolution of the oxide of gold, and thoroughly dried in a dark cloſet, was now expoſed, *in this dry ſtate,* to the action of the direct rays of a very bright ſun.

It had been expoſed to this ſtrong light above half an hour, before its colour began to be *ſenſibly changed*; and, at the end of three hours, it had acquired only a very faint violet hue.

Being now thoroughly wetted with diſtilled water, it changed colour very rapidly, and ſoon came to be of a deep purple tint, approaching to crimſon.

Experiment No. 8. A piece of white taffeta riband, which had been wetted with the ſolution, and thoroughly dried in the dark, was ſuſpended in a clean dry phial of very fine tranſparent glaſs; and the phial, being well ſtopped with a dry cork, was expoſed to the ſtrong light of a bright ſun.

After the riband had been expoſed, in this manner, to the action of the ſun's direct rays about half an hour, there were here and there ſome faint appearances of a change of its colour; but it ſhowed no diſpoſition to take that deep purple hue which the riband had always acquired, when expoſed to the light in the preceding experiments.

On taking the riband out of the phial, and wetting it thoroughly with diſtilled water, and expoſing it again, *while thus wetted,* to the ſun's rays, it almoſt inſtantly began to change colour, and ſoon became of a deep purple tint; but, though I examined the ſurface of the riband with the utmoſt care, and with a good lens, both during the experiment and after it, I could not perceive the ſmalleſt particle of *revived gold,* nor did I ſee any veſtige remaining that appeared to indicate that any had in fact been revived.

This experiment was repeated ſeveral times, and always with reſults which led me to conclude, (what indeed was reaſonable to expect,) that light has little effect in changing the colour of metallic oxides, *as long as they are in a ſtate of cryſtallization.*

The heat which is generated by the abſorption of the rays of light muſt neceſſarily, *at the moment of its generation* at leaſt, exiſt in almoſt infinitely ſmall ſpaces; and conſequently, it is only in bodies that are *inconceivably ſmall* that it can produce durable effects, in any degree indicative of its extreme intenſity.

Perhaps the particles of the oxide of gold diſſolved in water are of ſuch dimenſions; and it is very remarkable, that the colours produced, in ſome of my experiments on white ribands, by means of an aqueous ſolution of the oxide of gold, are preciſely the ſame as are produced from the oxide of that metal, by enamellers, in the intenſe heat of their furnaces.

As the colouring ſubſtance is the ſame, and as the colours produced are the ſame, why ſhould we not conclude that the effects are produced in both theſe caſes by the ſame means, that is to ſay, by the agency of heat? or, in other words, and to be more explicit, by expoſing the oxide in a certain temperature, at which it becomes diſpoſed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the reſults of the following experiments afford ſtill more ſatisfactory information, reſpecting the intenſity of the heat generated in all caſes where light is abſorbed, and the ſtriking effects which, under certain circumſtances, it is capable of producing.

The facility with which moſt of the metallic oxides are reduced, in the dry way, by means

means of charcoal, ſhows that, at a certain (high) temperature, oxygen is diſpoſed to quit thoſe metals, in order to form a chemical union with the charcoal, or at leaſt with ſome one of its conſtituent principles, if it be a compound ſubſtance; and hence I concluded, that gold might be revived, *in the moiſt way*, by means of charcoal, from a ſolution of its oxide in water, were it poſſible, under ſuch circumſtances, to communicate to the charcoal, and to the oxide, *at the ſame time*, a degree of heat ſufficient for that purpoſe.

To ſee if this might not be done by means of light, I made, or rather repeated, the following very intereſting experiment:

Experiment No. 9. Into a thin tube of very fine colourleſs glaſs, 10 inches long, and $\frac{6}{10}$ of an inch in diameter, cloſed hermetically at its lower end, I put as many pieces of charcoal, about the ſize of large peas, as filled the tube to the height of two inches; and, having poured on them as much of the aqueous ſolution of nitro-muriate of gold as nearly covered them, expoſed the tube, with its contents, to the action of the direct rays of a very bright ſun.

In leſs than half an hour, ſmall ſpecks of revived gold, in all its metallic ſplendour, began to make their appearance here and there on the ſurface of the charcoal; and, in ſix hours, the ſolution, which at firſt was of a bright yellow colour, became perfectly *colourleſs*, AND AS CLEAR AND TRANSPARENT AS THE PUREST WATER.

The ſurface of the charcoal was, in ſeveral places, nearly covered with ſmall particles of revived gold; and the inſide of the glaſs tube, in that part where it was in contact with the upper ſurface of the contained liquid, was moſt beautifully gilded.

This gilding of the tube was very ſplendid, when viewed by reflected light; but, when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greeniſh blue colour, without the ſmalleſt appearance of any metallic ſplendour.

From the colour, and apparent denſity of this cloud, I was induced to conclude, that the gilding on the glaſs was leſs than *one millionth part of an inch* in thickneſs.

This intereſting experiment was repeated ſix times, and always with nearly the ſame reſult. The gold was completely revived in each of them, and the ſolution left perfectly colourleſs: in moſt of the experiments, however, the ſides of the glaſs were not gilded, all the revived gold remaining attached to the ſurface of the charcoal.

In two of theſe experiments, I made uſe of pieces of charcoal which had been previouſly boiled ſeveral hours in a large quantity of diſtilled water, and which were introduced *wet*, and *hot*, into the tube, and immediately covered by the ſolution, to prevent them from imbibing any air; and, in different experiments, the ſolution was uſed of different degrees of ſtrength.

I plainly perceived that the experiment ſucceeded beſt, that is to ſay, that the gold was *ſooneſt revived*, in thoſe caſes in which the ſolution was *moſt diluted*: one of the experiments, however, and which ſucceeded perfectly, was made with the ſolution ſo much condenſed, that it was nearly at the point at which it became diſpoſed to cryſtallize *.

* This agrees perfectly with the reſults of ſimilar experiments made by the ingenious and lively Mrs. FULHAME. (See her Eſſay on Combuſtion, page 124.)
It was on reading her book that I was induced to engage in theſe inveſtigations; and it was by her experiments that moſt of the foregoing experiments were ſuggeſted. R.

On examining, with a good microfcope, the particles of revived gold which remained attached to the furface of the charcoal, after it had been dried, I found them to confift of an infinite number of fmall fcales, feparated from each other; not very highly polifhed, but poffeffing the true metallic fplendour, and a very deep and rich gold colour.

The gold which attached itfelf to the infide of the glafs tube, was in the form of a ring, about $\frac{1}{10}$ of an inch wide, (badly defined however below,) and adhered to the glafs with fo much obftinacy, as not to be removed by rinfing out the tube a great number of times with water; it had, as has already been obferved, a very high polifh, when feen by reflected light.

Thofe who enter into the fpirit of thefe inveftigations, will eafily imagine how impatient I muft have been, after feeing the refults of thefe experiments, to find out whether gold could be revived from this aqueous folution of its oxide by means of charcoal, *without the affiftance of light*, and merely by fuch a degree of equal heat as could be given to it in the dark. To determine that important queftion, the following experiment was made.

Experiment No. 10.—A cylindrical glafs tube, $\frac{6}{10}$ of an inch in diameter, and 10 inches long, clofed hermetically at its lower end, and containing a quantity of a diluted aqueous folution of the oxide of gold, mixed with charcoal in broken pieces, about the fize of large peas, was put into a fit cylindrical tin cafe, which was nicely clofed with a fit cover; and the glafs tube, with its contents, fo fhut up in the dark, was expofed two hours, in the temperature of 210° of Fahrenheit's fcale.

On taking the glafs tube out of its tin cafe, I found the folution *perfectly colourlefs*, and the revived gold adhering to the furface of the charcoal.

On repeating the experiment, and ufing the folution nearly faturated with the oxide, the refult was precifely the fame; the folution being found perfectly colourlefs, and the revived gold adhering to the furface of the charcoal.

I own fairly, that the refults of thefe experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypothefis, refpecting the caufes of the reduction of the oxide, in the foregoing experiments; but, whatever may be the fate of this, or of any other hypothefis of mine, I hope and truft that I never fhall be fo weak as to feel pain at the difcovery of truth, however contrary it may be to my expectations; and ftill lefs, to feel a fecret wifh to fupprefs experiments, merely becaufe their refults militate againft my fpeculative opinions.

It is proper I fhould obferve, that the charcoal ufed in this laft-mentioned experiment had been boiled two hours in diftilled water; by which means its pores had been fo completely filled with that fluid, that the pieces of it that were ufed were fpecifically heavier than water, and funk in it to the bottom of the containing veffel.

(To be continued.)

VI.

Experiments and Observations on the Nature of Sugar, and of Vegetable Mucilage. By Mr. WILLIAM CRUICKSHANK, *Chemist to the Ordnance, &c.**

FROM the failure of these trials to convert mucilages into something resembling sugar, we began to suspect that they were not so simple as had been generally supposed. With a view to throw some light on this subject, the following comparative experiments were made:

One ounce of powdered gum arabic was introduced into a coated glass retort, to which a receiver with the pneumato-chemical apparatus was adapted: heat being gradually applied, there came over into the receiver 3 dr. 30 gr. of pyromucous acid, mixed with a little heavy empyreumatic oil. After the retort had been red-hot for some time, it was removed, and the charry residuum which it contained was found to weigh 1 dr. 46 gr. This had a greyish colour, and burned very slowly; but when exposed to a strong heat in an open crucible, it left a whitish powder, amounting to 10 grains, which was found to be lime mixed with a very small proportion of calcareous phosphate.

The pyromucous acid being super-saturated with lime, a strong smell of ammonia was instantly perceived; and a piece of paper dipped in muriatic acid being held over the vessel, copious white fumes were immediately produced. This circumstance shews, that azote forms a constituent part of the gum.

There were collected in the pneumato-chemical apparatus 273 oz. measures of gas; of this 93 were carbonic acid, and the remaining 180, that species of hydro-carbonate which is obtained by heat from moistened charcoal.

Two measures of this gas, well freed from carbonic acid, were mixed with $1\frac{1}{2}$ of very pure oxygen gas, and introduced in a strong glass jar, filled with and inverted over mercury: when fired by the electric shock, they occupied the space of one measure only: lime water being admitted, the whole was absorbed, except a very small particle, which was found, from the nitrous test, to be pure air. From a number of experiments we have found, that twelve measures of oxygen gas, when united with carbon, produce ten of carbonic acid gas. Hence it follows, that the quantity of oxygen gas necessary to the formation of carbonic acid gas must, in this case, have been 1.1. measures, or a little better; the remaining 4 must therefore have been consumed in the production of water, and would be sufficient to saturate .8 of hydrogen, equal in weight to .048 of a grain nearly.

* These Experiments, &c. are a continuation of the Research communicated in the first volume of this Journal, page 337. In the second edition of Dr. Rollo's Treatise on Diabetes Mellitus in which that article is reprinted, and whence the above is taken, there are a few emendations in the second and third paragraphs, as follow: The sugar afforded $8\frac{1}{2}$ drams of acid, which required 150 grains of the alkaline solution to saturate it; the charry residue was 5 drams; and the gas which escaped $2\frac{1}{2}$ drams. The gum arabic afforded 7 dr. 40 gr. of acid, which required 118 grains of the solution of pot-ash to saturate it; the charry residue was 3 dr. 45 gr.; and the gas which escaped 5 drams. Whence the sugar yielded more pyromucous acid than the gum, in the proportion of 150 to 118.

The concluding paragraph on page 341, beginning, "*Indeed, when we reflect,*" &c. is cancelled, and the investigation is continued as in the text. The present article must therefore be read as a continuation from vol. i. p. 341, of this Journal. N.

Now,

Now, an ounce measure of carbonic acid gas weighs .864 of a grain, and this contains .24 of pure charcoal: hence the quantity of charcoal to hydrogen in this inflammable gas must be as .24 to .048, or 5 to 1.

But one measure of pure hydro-carbonate, such as may be obtained from the decomposition of camphor, by making its vapour pass through a red-hot earthen tube, or from the distillation of animal substances, opium, &c. requires two of pure air to saturate it; and the quantity of carbonic acid amounts to 1.45, which makes the proportion of carbon to hydrogen as 12 or 13 to 1. Hence these gases differ materially, and ought not to be confounded: indeed, the difference between them is manifest, from the manner in which they burn when mixed with common air, and brought into contact with an ignited body. Pure hydro-carbonate burns slowly, with a perfectly white flame, and never detonates: on the contrary, the compound inflammable gas just mentioned burns rapidly, with a reddish blue flame, and more or less of a detonation. We thought it necessary to mention this circumstance, as much confusion might be produced by applying the same name to substances considerably different. It is remarkable that æther, decomposed by heat, affords pure hydro-carbonate, whilst alcohol yields the mixed species.

But to return:—An ounce of gum tragacanth was submitted in a coated glass retort to a similar process, and the products collected were as follows:

	dr.	gr.
Charcoal remaining in the retort, - - -	1	45
Pyromucous acid, - - - - -	4	5
Carbonic acid gas, - - - -	78 oz. meas.	
Hydro-carbonate, - - - -	91 ditto.	

The charcoal burned slowly with a peculiar phosphorescent flame, and left a white residuum of 12 grains, which was found to consist of lime mixed with a little calcareous phosphate.

The pyromucous acid being super-saturated with lime, the quantity of ammonia disengaged appeared to be considerably greater than from that afforded by the gum arabic.

Having in this way discovered lime in both species of gum, we were anxious to know if this earth could be detected by reagents, without having recourse to decomposition by heat. Accordingly, a quantity of sulphuric acid was dropped into a solution of gum arabic: after standing for some hours, a number of needle-like crystals were slowly deposited: these being separated were dissolved in distilled water; to this solution the oxalate of ammonia was added, when a copious precipitate of oxalate of lime immediately took place.

From these experiments, therefore, it is manifest, that gums consist of oxygen, hydrogen, carbon, azote, and lime, with a little phosphoric acid.

An ounce of refined sugar was next introduced into a coated retort, and the pneumato-chemical apparatus applied, as in the former experiments. The products obtained were,

	oz.	dr.	gr.
Pyromucous acid, with a drop or two of empyreumatic oil,	0	4	30
Charcoal, - - - - - -	0	2	0
Hydro-carbonate, - - - - -	119 oz. meas.		
Carbonic acid, - - - - -	41 ditto.		

The pyromucous acid being supersaturated with lime, not the least vestige of ammonia could be perceived. Sugar, therefore, does not contain azote; neither does it contain lime;

 for

for the charcoal, which was of a beautifully black colour, burned out completely when exposed to a strong red heat.

Being desirous of ascertaining more completely the difference between common sugar and the saccharine matter secreted by the breasts of animals, an ounce of the crystallized sugar of milk was distilled in an apparatus similar to that already described, and the products were found to be,

	oz.	dr.	gr.
Pyromucous acid, mixed with a very little empyreumatic oil,	0	6	0
Charcoal, with a little phosphate of lime, - -	0	1	0
Carbonic acid gas, - - - -	31 oz. meas.		
Hydro-carbonate, of the same nature with that obtained in the former experiments, - - -	103 ditto.		

The charcoal being burned in an open crucible, there remained about one grain, which appeared to be phosphate of lime chiefly.

The pyromucous acid was next supersaturated with lime; but the quantity of ammonia disengaged was so small that it could with difficulty be detected: there appeared, however, to be a very little.

Hence, then, it would seem that this animal sugar, contrary to what might be expected, contains hardly any azote.

It would appear also, that it contains less charcoal, and more oxygen, than common sugar.

In order to investigate still further the nature of these substances, we endeavoured to ascertain the quantity of oxalic basis or radical contained in each, or how much oxalic acid they would afford when treated with an equal proportion of the nitrous acid.

An ounce of sugar was added to six ounces of the concentrated nitrous acid, diluted with an equal bulk of water. When the action had in a great measure ceased, heat was applied, and the evaporation continued until the liquor was reduced to about an ounce by measure: after this had cooled, the crystals were separated by filtration, and the remaining fluid again evaporated, until the whole, when cold, shot into a mass of crystals, leaving only a few drops which refused to crystallize. These crystals being collected, and well dried on blotting paper, amounted to 4 dr. 20 gr. or a little better than half the weight of the sugar employed.

An ounce of gum arabic was next treated with an equal proportion of nitrous acid properly diluted: the quantity of crystals collected amounted to 3 dr. 36 gr.; but in this instance, the last crystals obtained were mixed with an insoluble white powder, which, being separated from the oxalic acid by the addition of distilled water, amounted to 6 gr. and was found to be oxalate of lime. The pure acid, therefore, did not exceed 3 dr. and a half. An equal quantity of gum tragacanth afforded, by the same process, 3 dr. 10 gr. of oxalic acid, and 10 gr. oxalate of lime.

An ounce of the sugar of milk was also treated in a similar manner, with six ounces of the concentrated nitrous acid properly diluted, and there were obtained in all 3 dr. 48 gr. These crystals, however, were mixed with a white powder, which, being but a little soluble in water, was readily separated: it amounted to 30 gr. and appeared to be the saccholactic acid of Scheele; hence the pure oxalic acid did not exceed 3 dr. 18 gr.

An

An ounce of honey yielded, by a ſimilar proceſs, 4 dr. 4 gr. of pure oxalic acid, but exhibited no ſigns of ſaccholactic acid.

The following Table will ſhew, at one view, the different products reſulting from deſtructive diſtillation, with their relative proportions, and likewiſe thoſe obtained by the nitrous acid.

Subſtances employed, 1 oz. of each.	Products obtained by Heat.				Products obtained by Nitrous Acid.	
	Charcoal, &c.	Pyromucous Acid, &c.	Carbonic Acid Gas.	Hydrocarbonate.	Oxalic Acid.	Other Subſtances.
	oz. dr. gr.	oz. dr. gr.	oz. meaſures.	oz. meaſures.	oz. dr. gr.	
Sugar, - - -	0 2 0	0 4 30	41	119	0 4 20	None.
Honey, - -					0 4 4	None.
Sugar of Milk,	0 1 0	0 6 0	31	103	0 3 18	30 grains of ſaccholactic acid.
Gum Arabic, -	0 1 36 and lime 10 0 1 46	0 3 30 with ſome ammonia.	93	180	0 3 30	Oxalate of lime, 6 grains.
Gum Tragacanth,	0 1 33 and lime 12 0 1 45	0 4 5 with ſome ammonia.	78	91	0 3 10	Oxalate of lime, 10 grains.

From theſe experiments it would appear, that ſugar conſiſts merely of carbon, hydrogen, and oxygen; and that gum differs from ſugar, not only in containing a leſs proportion of oxygen, but alſo by its combination with lime and azote; and that the ſugar of milk differs from both, as it contains the radical of the ſaccholactic acid: in other reſpects, however, it approaches very nearly to the nature of vegetable ſugar. Does the milk of carnivorous and graminivorous animals yield the ſame proportion of this acid? and is this ſugar itſelf always of the ſame nature?

From the well known facts reſpecting vinous fermentation, there is now reaſon to ſuppoſe, that no ſubſtances, but thoſe which conſiſt ſimply of carbon, hydrogen, and oxygen, are ſuſceptible of it, and that an union with a fourth changes the nature of the compound ſo much as to render this proceſs impracticable.

With a view to throw ſome light on this obſcure ſubject, the following experiments were made:

Auguſt 4, 1798.—An ounce of ſugar diſſolved in 5 ounces of water was digeſted with a little freſh ſlaked quicklime, in a moderate heat, for about 15 minutes; the ſolution was then filtered, and about 2 drachms, by meaſure, of good yeaſt added; the veſſel and mixture, weighing 17 oz. 2 dr. were introduced below a large glaſs bell.

There was placed cloſe to it, as a ſtandard, another veſſel, containing an ounce of ſugar diſſolved in 5 ounces of warm water, and to which an equal quantity of yeaſt was added. This veſſel and mixture, which weighed together 17 oz. 1 drachm, 20 grains, were likewiſe placed

placed under a glaſs bell. The temperature of the room during the whole of this experiment was rather high, and varied from 68° to 75° of Fahrenheit.

The laſt mixture began to ferment in 12 hours, and in 24 the proceſs ſeemed very briſk, much water now condenſing on the ſides of the bell. In five days the proceſs began to ſubſide; in ſeven, the yeaſt fell to the bottom, and the liquor became clear. The mixture, at this time, ſmelled ſtrongly of beer turned a little ſour, although it ſtill taſted ſweetiſh. It was ſuffered to remain until the 28th, when it was removed from the bell and weighed, and the loſs was found to exceed three drachms: it now taſted much like a mixture of ſtrong vinegar and honey. During the whole of this period, amounting to 24 days, the mixture, containing the ſugar digeſted with the lime, never ſhewed the leaſt appearance of fermentation, nor was there any moiſture condenſed on the ſides of the bell. The veſſel and mixture being now weighed, the loſs amounted to two drachms nearly; the liquor ſmelled very muſty, and had a rough aſtringent taſte, mixed with ſweetneſs, but was not in the leaſt acid. In this inſtance the yeaſt fell to the bottom very ſoon, the liquor continued more or leſs mouldy, and became, at laſt, a little mouldy at the top.

We next digeſted an ounce of ſugar, diſſolved in five ounces of water, with a little pot-aſh, and to the filtered ſolution added about two drachms of very good yeaſt. This mixture was expoſed in an open veſſel to a temperature of about 65°; and another veſſel, containing a ſolution of an equal quantity of ſugar, mixed with yeaſt, placed by it as a ſtandard.

The ſolution with the yeaſt alone began to ferment in 12 hours; but the other, containing the pot-aſh, ſhewed no ſymptoms of fermentation at the end of 12 days.

Being now in a great meaſure ſatisfied, that any fourth ſubſtance combined, although in ſmall quantity, with the three which form ſugar, would prevent fermentation, we next wiſhed to know if every compound, conſiſting of carbon, hydrogen, and oxygen, however differently theſe ſubſtances might be proportioned, were ſuſceptible of its proceſs. The ſugar of milk, from what has already been obſerved, evidently conſiſts of theſe three ſimple ſubſtances; but, from a number of its ſenſible properties, and the reſult of its analyſis by heat, as well as its containing the radical of the ſaccholactic acid, it is manifeſt that their combinations and proportions muſt be very different from thoſe in common ſugar.

We therefore mixed a ſolution of its ſaccharine matter with the uſual quantity of yeaſt, and expoſed it to a temperature ranging from 65° to 70°: in four days ſome degree of fermentation was perceptible, and in three days more, became briſk: at the end of ſixteen, when its proceſs had apparently ceaſed, the liquor was examined: it now had the ſmell of cyder, but rather more of the flavour of apples; to the taſte it was very ſour, and, when added to the tincture of litmus, ſtrongly reddened it.

The acid thus procured either contained or conſiſted of common vinegar; for with the oxyde of lead it formed a ſweet ſaline maſs, compoſed evidently of ſlender priſmatic cryſtals which were not deliqueſcent.*

Hence it is manifeſt, that the ſugar of milk is at leaſt in ſome degree ſuſceptible of the vinous fermentation.

Having obſerved, in our attempts to convert gum into ſugar, that it ſeemed to run eaſily into the acid ſtate, we were anxious to know if any thing like fermentation preceded this

* See Scheele's Eſſays, page 274.

ſtate,

ftate, or if it was poffible to convert it into an acid, by mere expofure to the air, without the addition of fome fubftance containing much oxygen, as the nitrous or oxygenated muriatic acids. Accordingly a folution of gum arabic, mixed with a proper proportion of good yeaft, was introduced into an open veffel, and kept at a temperature ranging from 68° to 75° for twenty-fix days, but during this period nothing like fermentation was perceived: the mixture at laft emitted a very peculiar and offenfive fmell: the gum, however, ftill retained its natural tafte, and was not in the leaft four. In this cafe it fhould appear, that the azote, and lime, which in the gum are combined with carbon, hydrogen, and oxygen, prevented the vinous fermentation, and confequently the formation of any thing like vinegar.

It has been fuppofed, that a decoction of purely animal matter might undergo certain fpontaneous changes, and at laft become acid. In order to determine this point, about fixteen ounces of a ftrong decoction of beef were introduced into an open veffel, and kept at the temperature of about 68° or 70°. A few days after, an equal quantity of a fimilar decoction, mixed with an ounce of yeaft, was likewife expofed in an open veffel to air of the fame temperature.

At the end of five days, the decoction, without any mixture, began to fhew evident marks of putrefaction, but did not in the leaft tafte acid, nor had it ever fhewn any figns of vinous fermentation; in two days more it became extremely offenfive, accompanied with the production of ammonia.

The decoction with the yeaft did not fhew any evident figns of putrefaction until the feventh day, but there was nothing like fermentation perceived; in two days more it became extremely putrid and offenfive, and was thrown away.

Hence it fhould appear, that neither vegetable nor animal mucilages are, when pure, in any degree fufceptible of the vinous fermentation: indeed, thefe are facts which have been fo generally admitted, that, had not a contrary opinion been lately advanced, we fhould have conceived the three laft experiments unneceffary.

The miftake has no doubt arifen from obferving the facility with which a decoction of a mixture of animal and vegetable matter runs into the acid ftate: but in this cafe the animal fubftance performs the part of yeaft only; and it is in this way that diabetic urine fo readily ferments fpontaneoufly, and becomes vinegar.

From the preceding experiments we may draw the following conclufions:

1ft. That fugar confifts of carbon, hydrogen, and oxygen; and may be confidered as a pure vegetable oxyde.

2d. That fugar of milk is compofed of the fame principles, but contains more oxygen and confiderably lefs charcoal.

3d. That gum differs from fugar in containing, befides carbon, hydrogen, and oxygen, both lime and azote.

4th. That vegetable farina cannot be converted into faccharine matter, without the joint action of oxygen and water; the firft of which appears to be abforbed, and the laft decompofed, during this procefs.

5th. That when fugar is deprived of its oxygen, or combined with other fubftances, it lofes its characteriftic properties, and is no longer fufceptible of the vinous fermentation.

6th. That neither vegetable nor animal mucilages, in their pure ftate, are fufceptible of this procefs.

VII. *On*

VII.

On the Art of covering Wire Cloth with a transparent Varnish, as a Substitute for Horn; and on other Objects of public Utility. By ALEXIS ROCHON, *of the National Institute of France, &c.**

IN the progress of the present war, the marine store-houses of France were totally without the essential article of horns for lanthorns. It was impossible to substitute glass in the place of this article, on account of its brittleness, and the obvious danger which might result from that quality. In this situation of distress, the agents of the French government consulted Citizen Rochon, and directed him to make every experiment he could think of to discover a proper substitute. His attention was first directed to a memoir of the celebrated Poivre on the fabrication of lanthorns of horn by the Chinese. It is known that this industrious nation prefer horn to glass on account of its cheapness and toughness, and that they possess the art of welding this substance together with so much delicacy, that they make lanthorns of two feet diameter of astonishing transparency, and to all appearance of one single piece. It is also known that the Chinese use the horns of goats and sheep only, which they soften and split into laminæ by processes supposed to be unknown in Europe; or, perhaps, by employing a proportion of human labour and patience for that purpose which the European demand might be inadequate to repay. Citizen Rochon, who does not appear to be perfectly aware of the degree of accuracy with which the same art of splitting horn is practised in Europe, proposed, that the horns of beeves should be sawed into laminæ, and then scraped and polished; or, to which he gives the preference, that they should be laminated in boiling water.

While this active philosopher was employed at Brest in establishing a manufactory for laminating the horns of beeves, which he purposed to reduce into the state of a paste by means of pure alkali in the digester of Papin, it occurred to him, that he might supply the pressing wants of the navy by another expedient, which consisted in the application of a coating of glue upon wire cloth.

In this process, he at first tinned the iron wires of the sieve cloth he made use of, but afterwards found it more convenient, in every respect, to give it a slight coating of oil paint to preserve it from rust. The glue he made use of was afforded by boiling the clippings of parchment with the air-bladders and membranes of sea-fish; materials which he used, not from any notion that they were preferable to isinglass, but because they were the cheapest he could procure. He added the juice of garlick and cyder to his composition, in proportions which, I suppose, he did not measure, but which he found to communicate great tenacity and somewhat more of transparence than it would have possessed without them. Into this transparent and very pure glue or size he plunged his wire cloth, which came out with its interstices filled with the compound. It is requisite that the size should possess a determinate heat and consistence, concerning which, experience alone must guide the operator.

* Extracted from a memoir read to the National Institute of France the 21st Ventose, in the year VI. (March 11th, 1798), and inserted in the Journal de Physique for April 1798. The memoir contains various political and economical observations more particularly applicable to France, with general observations, which I have not thought it necessary either to transcribe or abridge; neither have I been solicitous to take the very words of my author in the parts I have abstracted. N.

When

When this prepared wire cloth is fixed in the lanthorn, it muſt be defended from moiſture by a coating of pure drying linſeed oil; but even in this ſtate it is not fit to be expoſed to the weather. The eaſe with which theſe lanthorns are repaired in caſe of accident, by a ſlight coating of glue, is pointed out as a great advantage by the inventor, who likewiſe informs us that they were uſed in the expedition to Ireland as ſignal lanthorns, though contrary to his wiſhes. For this uſe he recommends the large plates of mica, which were then imported from Boſton. With the latter ſubſtance, encloſed between two pieces of very open wire cloth, he made certain ſquares, 26 inches in length and 18 in width, for the light-houſe at Uſhant, which had been damaged by a flock of wild ducks, that flew through the windows and daſhed out the lights.

Citizen Rochon affirms, that lanthorns of wire cloth, prepared in his method, are much cheaper than thoſe made of tin and horn; that they are very cheaply repaired, and afford a ſtronger light.

He applied coarſe iron wire cloth to another uſe, which, he thinks, may be worth attention in future. He made the roof of one of his workſhops of this wire-cloth in order to avoid the danger of fire, and covered it with a ſlight coating of plaiſter. He thinks that a compoſition of lime and pounded ſcales of iron would have been preferable. This coating ought not to be thicker than a ſlate; and he recommends that it ſhould be penetrated with boiling whale oil, and painted with tar and ochre. Such a roof would afford no hold for the wind, and might, as he apprehends, be of conſiderable uſe to defend buildings and ſheds which require particular defence againſt fire.

In the courſe of experiments made for the diſcovery of a varniſh proper to defend his new lanthorns from moiſture, Citizen Rochon did not employ reſins or copal, which are always ſomewhat friable, but a perfect ſolution of elaſtic gum in drying linſeed oil. This varniſh or unguent having fixed the attention of Genouin, that learned chemiſt demanded whether it might not be practicable to uſe it in making bougies and other medical inſtruments, which were alſo at that time very much wanted. From this ſuggeſtion, Rochon employed the Engliſh machine for weaving whips to make the more conſiſtent part of the inſtrument. He plunged this woven piece in a mixture of melted wax with a little ochre, then drew it through a wire plate to take off the ſuperfluous wax, and render it perfectly ſmooth; after which he applied the varniſh of elaſtic gum, which completed the inſtrument.

Sartori, ornamental painter at Breſt, pointed out to our operator that fiſh glue is preferable to parchment ſize upon open wire cloth, becauſe it is more tranſparent and ſtronger.

VIII.

On the Production of Nitric Acid by the Contact of Oxygen very much heated and the Air of the Atmoſphere.*

PAUL, a celebrated artiſt at Geneva, having conſtructed and improved the apparatus of Watt for the production of the gaſes, was employed in obtaining by means of this apparatus the oxygen gas from the black oxide of mangaueſe, taking care not to cloſe the apparatus till the manganeſe was red-hot, in order to permit the eſcape of the water and carbonic acid

* Communicated to Dr. Delametherie by J. L. Odier. Journal de Phyſique, iii. New Series, p. 464.

which this oxide always contains in its native ftate. While the gas was extricated in great abundance, one of the cocks of the tube through which the gas paffed was accidentally opened, fo that a portion of the gas, very hot and pure, was emitted into the atmofphere. At the fame inftant all the affiftants were furprifed by a manifeft fmell of nitric acid, and a flight fume was feen to rife from the place whence the gas had iffued.

Profeffor Pictet, who was prefent, firft took notice of the fingularity and importance of this fact. It appears to prove, that when hot and very pure oxygen is brought into contact with the atmofpheric air at the ordinary temperature, nitric acid is formed by the chemical combination of the two conftituent principles of that acid; fo that by caufing pure and hot oxygen gas on the one hand, and atmofpheric air on the other, to pafs into a glafs globe or any other clofe veffel, a great quantity of nitric acid gas would be obtained, which might be condenfed and abforbed by water previoufly put into the veffel. Again, it is known that manganefe has the property of abforbing the oxygen of the air, or of water, when deprived of its original portion by means of fire. It might therefore be practicable, by means of a determinate quantity of the black oxide of manganefe, to obtain fucceffively from the atmofpheric air itfelf an unlimited quantity of nitric acid, which would befides have the advantage of being perfectly pure, if care was taken to purify the atmofpheric air made ufe of in the operation*.

IX.

Analyfis of the Chryfolite of the Jewellers, proving it to be Phofphate of Lime. By Citizen VAUQUELIN†.

WHEN I frequently heard from the mouth of Fourcroy, in his lectures during the laft ten years, and repeated in my memoir on the new metal contained in native red lead, that, if the art of the chemift could be exercifed upon the objects of natural hiftory preferved in collections, difcoveries would often be made of much utility to the advancement of that fcience; I did not expect that I fhould have fo fpeedy an opportunity of evincing the truth of that obfervation to the Inftitute.

Naturalifts have hitherto confidered the chryfolite as a precious ftone of the fecond order, and all writers have arranged it at the end of the gems properly fo called. Citizen Lametherie, in his Manuel du Minéralogifte, placed it immediately after the emerald and the aqua marine; Citizen Sage, in his Chemical Analyfis, has ranged it with the faphir;

* This fact appears to deferve the notice which the Author and the worthy Genevan Profeffor have beftowed upon it, and is certainly entitled to farther inveftigation. In order that the fpeculations at the end of the paper may be admitted or refuted, it appears neceffary to determine, whether the elaftic fluid which efcaped was pure oxygen, or was contaminated with nitric acid. This might have been afcertained by examining the water over which the gas was collected. As the native oxide of manganefe contains azote, which, as Fourcroy informs us, is driven over, for the moft part, before the ignition; and as Milner and Cavendifh have fhewn that nitrous acid is formed by the combination of oxygen and azote at a red heat, there feems to be fome ground for fufpicion, that the nitrous acid in the cafe before us was formed by a remaining portion of azote in the manganefe after the ignition took place. N.

† Read to the firft clafs of the National Inftitute of France, in Brumaire, in the year VI. (Nov. 1797.) It is inferted in the Annales de Chimie, xxvi. 123. I have added the words in the title expreffing the component parts of this ftone. N.

and

and Wallerius has placed it between the emerald and the garnet. The account of this author is copied in the note below*.

Kirwan uſes the term chryſolite to denote merely the peridot, which nevertheleſs differs from it greatly in the nature of its principles.

Achard of Berlin analyſed a ſpecies of chryſolite in which he affirms that he found ſilex 0.15; alumine 0.64; lime 0.17; iron 0.1.

But the reſults of this analyſis are ſo different from mine, that I ſtrongly ſuſpect he muſt have operated on a different ſtone from the true chryſolite. This ſuſpicion is ſo much the more probable, as the name of chryſolite has been given to many different ſtones, particularly the peridot, the chryſo-beryl, the olivine, and in general all ſtones which have a greeniſh yellow colour.

Citizen Launoy, in a journey he is at preſent performing in Spain to collect the objects of natural hiſtory, found in the hands of a dealer a conſiderable quantity of chryſolites, which he ſent to Paris; and the Council of Mines having purchaſed part of them, directed me to ſubmit them to analyſis.

It was not long before I diſcovered that this foſſil, which has all the external appearances of a ſtone, is not truly of that claſs; but that, on the contrary, it is a ſalt compoſed of an acid and a baſe which are both well known; namely, the phoſphoric acid and lime.

As ſoon as I had obtained this firſt reſult I enquired of Citizen Hauy, whether he had compared the maſſes of chryſolite with thoſe of the apatite or cryſtalliſed phoſphate of lime. He anſwered, that he had not, but that he had among his papers the relative reſults of the primitive forms of both, and would immediately compare them. He found with pleaſure that they did not differ in the leaſt appreciable quantity; and this concluſion may even be drawn from the reſults of which he has given an abſtract in his treatiſe publiſhed in the Journal des Mines. Citizen Hauy, therefore, diſcovered by geometry what I confirmed by chemical analyſis; and this ſatisfactory agreement, between two ſciences apparently ſo remote from each other, affords a proof of the truth and certainty of their reſpective principles.

I ſhall now proceed to relate the experiments by means of which I aſcertained the nature of the principles of the chryſolite, and determined their proportions.

Experiment 1. Two hundred parts of chryſolite in cryſtals being ſubjected to the action of a ſtrong heat for one hour, loſt their yellow colour without undergoing any change of form or tranſparency. They then reſembled rock cryſtal, and had loſt only one two-hundredth part of their weight.

Experiment 2. One hundred parts of pulveriſed chryſolite were mixed with the ſame quantity of concentrated ſulphuric acid, and about four hundred parts of diſtilled water; the mixture immediately became hot, and aſſumed the conſiſtence of thick ſoup. Nearly the ſame quantity of water as before was added, and the whole was boiled for ſeveral hours in a matraſs with a long neck. The mixture being then diluted with much water

* "Colore hæc gemma gramineo viridi flavo, ſeu aurantiorum, omnibus gemmis imo cryſtallo montano mollior, chalybe raſilis, calcinata colorum tranſparentiam perdit, albeſcens, pondere et parum diminuitur; certis circumſtantiis per ſe liquabilis in vitrum opacum album; in eo etiam à reliquis gemmis diſtincta, quòd in momento fuſionis eodem modo phoſphoreſcat ut terra aluminaris, vel ſpathum gypſoſum. Cum borace inſtar ſmaragdi in fuſionibus ſe habet, quam tamen gravitate ſpecificâ ſuperat in proportione ad aquam ut 3.600, vel 3.700 : 1.000. Figura dicitur eſſe polygona ſeu quadrangularis; occurrit etiam ſiliciformis, rotundata, in Braſiliâ."

was filtered; and the solid matter being collected, washed, and ignited in a silver crucible, weighed 116 parts. This matter, subjected to various proofs, exhibited all the characters of sulphate of lime.

The filtered liquor was evaporated to dryness in order to expel the sulphuric acid. The residue was ductile, and might be drawn out into threads as long as it preserved its heat; but it hardened by cooling like a kind of glass slightly opaque; it weighed 46 parts. These forty six parts, dissolved in water and mixed with carbonate of ammoniac, formed an abundant precipitate which became still more abundant by heat. This precipitate separated by the filter, washed, and ignited in a silver crucible, weighed 11 parts. It was phosphate of lime not decomposed.

The fluid thus cleared of the phosphate of lime was evaporated to the consistence of a syrup. At the expiration of several days it afforded crystals in the form of a four-sided prism terminating in quadrangular pyramids, whose sides corresponded with those of the prism, and of a penetrating urinous taste. Heat decomposed this salt, ammoniac was driven off, and the acid remained in the state of a perfectly transparent glass. The glass, mixed with the powder of charcoal and strongly heated in a retort, very speedily afforded phosphorus.

It cannot be doubted, therefore, but that this pretended stone is a combination of lime and phosphoric acid; a true native phosphate of lime in crystals. Nevertheless, though I am convinced by experience of the accuracy of the result I have announced, I was desirous of making some other experiments in order that there might not be the slightest doubt upon the subject.

Experiment 3. One hundred parts of the same substance in powder were digested in muriatic acid diluted with two or three parts of water. The solution took place speedily without the least effervescence; it was clear and colourless.

This solution, evaporated nearly to dryness in order to dispel the excess of muriatic acid, was again diluted with water, and mixed with a solution of oxalic acid, till the precipitate which immediately followed was no longer produced. The filtered liquor left upon the paper a precipitate, which after washing and drying weighed 118 parts. These 118 parts of precipitate, strongly calcined in a crucible, assumed at first a black colour arising from the coal of the oxalic acid decomposed by the fire. This coal being burned off, there remained 54,28 parts of a white pulverulent substance, which was acrid, soluble in water, turned vegetable blue colours to a green, and, in a word, presented all the characters of pure lime. The fluid from which the oxalate of lime had been separated was evaporated to dryness, and assumed a black colour on account of an excess of oxalic acid which the heat had decomposed.

When the whole of this last acid appeared to have been entirely converted into carbon, the residue was dissolved in water, and the solution filtered in order to separate the carbon. The fluid part, saturated with carbonate of ammoniac, let fall a few light flocks of phosphate of lime which weighed one part. This fluid, subjected to evaporation, afforded a salt perfectly resembling that of the former experiment. It was decomposed by lime water, and was fused with the blow-pipe, with inflation, and emitted a smell of ammoniac and a yellow greenish light. The residue was a transparent glass.

Since, therefore, this experiment perfectly agrees with the former, not only with regard to the nature of the principles of the chrysolite, but also the proportions, as will immediately

diately be ſhewn; it would be uſeleſs to add to the number of experiments, which would convey no other information than is already known concerning the phoſphate of lime.

In order to eſtabliſh the proportions of phoſphoric acid and lime in the chryſolite, it muſt be recollected, 1. that 100 parts of this ſubſtance afforded in the ſecond experiment 116 parts of calcined ſulphate of lime, which, according to Bergman, contained 48,84 of pure lime: 2. that there remained 11 parts of phoſphate of lime not decompoſed, which were capable of forming 14,33 of ſulphate of lime, which added to the 116 give 130,33. Now if 116 contain 48,84 of lime, it is evident that 130,33 muſt contain 53,32. According to this experiment, therefore, 100 parts of chryſolite contain 53,32 of lime; and ſubtracting this from the 100, there will remain for the phoſphoric acid 46,68. In the third experiment it is alſo ſeen, that 100 parts of chryſolite, diſſolved in the muriatic acid, afforded by the oxalic acid 118 parts of oxalate of lime; and that theſe 118 parts left after calcination 54,28 parts of pure lime, which ſubtracted from 100, give 45,72 for the phoſphoric acid.

We ſee, therefore, that the reſults of theſe two experiments do not differ ſo much as one hundredth part from each other, and that they perfectly agree with the component parts obtained by Klaproth in his analyſis of the apatite, from which he obtained 55 parts of lime and 45 of phoſphoric acid.

X.

Account of a ſingular Inſtance af Atmoſpherical Refraction. In a Letter from WILLIAM LATHAM, *Eſq. F.R.S. and A.S. to the Rev.* H. WHITFIELD, *D.D. F.R.S. and A.S.**

DEAR SIR, Haſtings, Auguſt 1, 1797.

ON Wedneſday laſt, July 26, about five o'clock in the afternoon, whilſt I was ſitting in my dining-room at this place, which is ſituated upon the parade cloſe to the ſea-ſhore, nearly fronting the ſouth, my attention was excited by a great number of people running down to the ſea-ſide. Upon enquiring the reaſon, I was informed that the coaſt of France was plainly to be diſtinguiſhed with the naked eye. I immediately went down to the ſhore, and was ſurpriſed to find that, even without the aſſiſtance of a teleſcope, I could very plainly ſee the cliffs on the oppoſite coaſt; which, at the neareſt part, are between forty and fifty miles diſtant, and are not to be diſcerned, from that low ſituation, by the aid of the beſt glaſſes. They appeared to be only a few miles off, and ſeemed to extend for ſome leagues along the coaſt. I purſued my walk along the ſhore to the eaſtward, cloſe to the water's edge, converſing with the ſailors and fiſhermen upon the ſubject. They, at firſt, could not be perſuaded of the reality of the appearance; but they ſoon became ſo thoroughly convinced, by the cliffs gradually appearing more elevated, and approaching nearer, as it were, that they pointed out, and named to me, the different places they had been accuſtomed to viſit; ſuch as, the Bay, the Old Head or Man, the Windmill, &c. at Boulogne; St. Vallery, and other places on the coaſt of Picardy; which they afterwards confirmed, when they viewed them through their teleſcopes. Their obſervations were, that the places appeared as near as if they were ſailing, at a ſmall diſtance, into the harbours.

* Philoſ. Tranſ. 1798, page 357.

Having

Having indulged my curiosity upon the shore for near an hour, during which the cliffs appeared to be at some times more bright and near, at others more faint and at a greater distance, but never out of sight, I went upon the eastern cliff or hill, which is of a very considerable height, when a most beautiful scene presented itself to my view; for I could at once see Dengeness, Dover cliffs, and the French coast, all along from Calais, Boulogne, &c. to St. Vallery; and, as some of the fishermen affirmed, as far to the westward even as Dieppe. By the telescope, the French fishing-boats were plainly to be seen at anchor; and the different colours of the land upon the heights, together with the buildings, were perfectly discernible. This curious phenomenon continued in the highest splendour till past eight o'clock (although a black cloud totally obscured the face of the sun for some time), when it gradually vanished.

Now, Sir, as I was assured, from every enquiry I could possibly make, that so remarkable an instance of atmospherical refraction had never been witnessed by the oldest inhabitant of Hastings, nor by any of the numerous visitors (it happening to be the day of the great annual fair, called Rock fair, which always attracts multitudes from the neighbouring places), I thought an account of it, however trifling, would be gratifying to you.

I should observe, the day was extremely hot, as you will perceive by the subjoined rough journal of a small thermometer, which was kept in the dining-room above mentioned. I had no barometer with me, but suppose the mercury must have been high, as that and the three preceding days were remarkably fine and clear. To the best of my recollection, it was high water at Hastings about two o'clock P.M. Not a breath of wind was stirring the whole of the day; but the small pennons at the mast-heads of the fishing-boats in the harbour were, in the morning, at all points of the compass.

I am, &c.

WILLIAM LATHAM.

P. S. I forgot to mention, that I was a few days afterwards at Winchelsea, and at several places along the coast; where I was informed the above phenomenon had been equally visible. I should also have observed, that when I was upon the eastern hill, the cape of land called Dengeness, which extends nearly two miles into the sea, and is about sixteen miles distant from Hastings, in a right line, appeared as if quite close to it; as did the fishing-boats, and other vessels, which were sailing between the two places: they were likewise magnified to a great degree*.

* On this interesting subject see Mr. Huddart's Observations on Horizontal Refractions, Philos. Journal, I. 145. Ellicot, on the Phenomenon of Looming, I. 152. The *Fata Morgana*, or Appearance of Figures in the Sea and Air, before Reggio, described, I. 225. Mudge, in the Philos. Trans. 1795, p. 586, 587. Smeaton's Account of Eddystone Light-House, p. 191. Hutton's Dictionary, II. 352. These appearances are much more frequent and general than has usually been supposed. I have been credibly informed, that the *Fata Morgana* has been seen from Broad Stairs, in Kent; and that the elevation and inversion of terrestrial objects is commonly or frequently observable (in summer, I suppose), through a telescope, over level ground, if the eye of the observer be not much elevated. In the curious instance related above by Mr. Latham, it does not seem probable that the French coast was enlarged, but that it was only elevated by the refraction. At all events, the objects could scarcely have suffered any alteration of the horizontal angles or bearings. N.

STATE

STATE *of the* THERMOMETER *at HASTINGS, during the Month of* JULY 1797.

1797.	Therm.	Time.	Wind.	Weather.
July 1.	64	10 A. M.	SW	Windy. Fair.
2.	64	10	SW	Windy. Fair.
3.	62	10	SW	Rain. Windy.
4.	62	10	SW	Fair. Windy.
5.	61	10	SW	Rain. Windy.
6.	60	10	SW	Rain. Windy.
7.	61	10	W	Rain. Windy.
8.	62	10	NW	Fine.
	66	5 P. M.	NW	Fine.
9.	66	10 A. M.	SW	Fine.
10.	67	10	N afterw. SW	Fine.
11.	65	10	SW	Foggy all day.
12.	63	10	SW	Fine.
13.	72	10	SW	Fine.
14.	76	10	W	Fine.
	68	12	W	Fine.
15.	72	10	W	Fine.
16.	72	10	N	Fine.
	78	7 P. M.	E	Storm of Wind. Lightning.
17.	73	10 A. M.	W	Fine.
18.	70	10	W	Fine. Showers in the Night.
19.	67	10	WSW	Fine. Windy.
20.	67	10	SW	Rain. Windy.
21.	65	10	SW	Fine. Windy.
22.	61	10	S	Rain.
23.	65	10	S	Fine.
24.	66	10	S	Fine.
25.	66	10	SW	Fine.
26.	68	10	SW	Fine. Dead calm all day.
	76	5 P. M.	SW	Fine.
27.	72	10 A. M.	SW	Fine.
28.	70	10	S	Fine.
29.	72	10	E	Fine.
30.	70	10	SW	Rain.
31.	69	10	S	Fine. Windy.

XI.

An Account of Electrical Machines of considerable Power, in which Silk is used instead of Glass. W. N.

ELECTRICITY has been, in most practical cases, excited or collected by the friction of various substances against each other. In this process, which has not yet been explained by reference to simpler phenomena, it is a known condition, that one at least of the substances rubbed must be a non-conductor. Resin, lack, silk, baked wood, and above all glass, are the bodies which have hitherto been used. The durability and unchangeable nature of glass*, and its being very little if at all affected by the atmospherical variations, are undoubtedly the causes why it has obtained the preference. Its brittleness, and the great expence of large plates or cylinders, are certainly among those reasons why philosophical operators should be desirous of a substitute of less cost and danger.

Dr. Ingenhousz, the inventor of the plate machine, made a variety of experiments for this purpose. Pasteboard thoroughly dried and heated, and then soaked and varnished with a solution of amber in linseed oil, formed plates which were strongly electrified when rubbed with a cat's skin or hare's skin. He tried baked wood boiled in linseed oil, but with less success. A cylinder of strong silk velvet, formed by stretching that substance upon two circular wooden disks, was found to afford considerable electrical force when caused to revolve against a cushion covered with hare's skin†. And lastly, the same philosopher contrived a portable apparatus for charging a jar by means of a varnished silk riband, exposed to the friction of a rubber attached to the external coating, while the opposite electricity of the silk was taken off by a metallic part communicating with the inside‡.

It was at the beginning of 1784 that M. Walckiers de St. Amand undertook to construct a machine, in which a piece of silk was made to revolve incessantly, and pass between two pair of rubbers. He made one of small dimensions, and afterwards a larger in which the silk was twenty-five feet in length, and five feet broad. In the following year a machine of the same kind was constructed by M. Rouland, professor and lecturer in natural philosophy in the university of Paris §. As no accounts of either of these have been published in this country ||, and the advantages and effects described by the authors appear

* Mr. C. Cuypers of Delft affirms that glass becomes harder, and fitter for electrical purposes, by long exposure to the warm air of a room; and Mr. Birch, of Essex-street, in a very extensive electrical practice, found that glass cylinders lose their power by long use, so as to become of no value; but he ascribes this change to the use of the aurum musivum on the cushion.

† Bakerian Lecture, Phil. Trans. 1779.

‡ Nouvelles Expériences, &c. sur la Physique, par J. Ingenhousz, F.R.S. &c. Paris, 1785.

§ Description des Machines electriques à taffetas, par M. Rouland, &c. Amsterdam and Paris 1785, octavo, 35 pages, with one plate, of which Pl. XVIII. is a copy. — The Report of the Parisian Academy on the Machine of M. Walckiers, dated 25th March 1784, was separately printed in 29 pages octavo, with a coloured plate. The Report is copied in M. Rouland's pamphlet.

|| Some time after the construction of these machines, Mr. Edward Nairne, of Cornhill, whose researches in this and other practical departments of science are well known, received an order to construct one of the same form, but found it impossible by any adjustment to prevent the silk from running totally to one or the other end of

appear to be considerable, I have thought it of advantage to insert the description of the latter in this place.

A, B, Pl. XVIII. is a wooden table four feet and a half long, two feet nine inches wide, and somewhat more than an inch and a half thick: its feet are 18 inches long. Upon this table are fastened by strong wooden screws, *a b c d*, two cross pieces, each nine inches broad, which carry the uprights C, D, E, F, which last are 27 inches in height. At about two-thirds or more of the height of these uprights, there are cut notches of an inch square each, in which the axes of the two cylinders G and H turn freely. These axes are parallel to the table and to each other, and are kept in their place by clamps of wood screwed over them. The cylinders G and H are formed of light wood glued together, and covered at the ends by a circular piece, whose rounded edges arise half an inch above the surface of the cylinders themselves. Their diameter is eight inches; the axes are of box wood, and are less than an inch in diameter, having a shoulder which prevents the ends of the cylinders from touching the uprights when turned round; and lastly, the cylinders are covered with serge.

The handle is copper, its radius being six inches long.

K, L, is a piece of taffety covered with oily and resinous matter, of the same kind as is used in France in the construction of air-balloons, which, M. Rouland says, renders the silk very electrical: the breadth of the silk is nearly one inch less than the length of the cylinders, and it is wrapped round them with its ends sewed together.

The whole breadth of the silk is taken hold of or pinched between two flattened tin tubes opposite each other at M, and two of the same kind at N: these are the rubbers, and may be made to press against each other, more or less strongly, by means of screws. They are retained by strings of silk fastened to the four uprights of the machine. *v v* are two brass chains hooked upon the rubbers, and communicating with the earth; *o p* and *q r* are four pieces of taffety, prepared in the same manner as the principal piece, sewed in the direction of their length to the rubbers, and fastened to each other by their corresponding corners by means of threads of silk. The metallic tubes or rubbers are covered with cat's skin.

S represents the conductor. It is a cylinder of brass three inches in diameter, 36 inches in length, including the balls at the end, whose diameters are four inches: one of these balls has a ring, *t*, above it, which serves to form a communication between the conductor S and any other conductor.

The upper and lower parts of this cylindrical prime conductor are armed with two plates of brass *y y*, whose length is equal and correspondent to the breadth of the taffety, which is 26 inches, and 132 inches or 11 feet long: the edges of the plates are about half an inch distant from the silk, and serve instead of the metallic points that were used by M. Walckiers, but rejected by M. Rouland, because they were apt to stick into the silk and damage it.

of the cylinders. The inventors have given no instructions to obviate this defect. I have no doubt but that they made their rollers gradually largest in the middle. I have seen a machine for folding woollens, invented and made by Mr. Rehe, of Shoe-lane, in which this difficulty was removed in some leading rollers, by making them in the form of two very acute frustums of cones, joined at the middle by the larger base.—See Phil. Journal, I. 23.

The conductor S is suspended by silk strings, fastened to the uprights of the machine by the hooks and rings *i i*: its situation is parallel to the cylinders G, H, and equidistant from each. The action of this machine is as follows: The cylinder H is moved rapidly on its axis by means of the handle, and the cylinder G moves of course in the same direction on the two extremities of its axis, provided the taffety K, L, be properly stretched. This tension is easily obtained; because the cross pieces to which the uprights C, D, and E, F, are fixed, may be moved nearer or further from each other, and fastened by means of the screws *a b* and *c d*, which pass through holes cut in the direction of the table.

The rotation of the cylinders necessarily producing a circulation of the taffety, it must consequently be rubbed in its passage between the tin tubes covered with cat's skin at M and N; and by this friction it obtains what is called the negative electricity, which is communicated from both parts of the silk to the common conductor S. But it may be made to electrify positively, by removing the rubbers to the middle of the silk, so that the prime conductor may communicate with them: or, if the two cushions be removed to half the distance between the revolving cylinders and the prime conductor, positive and negative electricity may be had at the same time, the rubbers being in a negative state, and the prime conductor in a positive state.

The advantages of a machine of this construction beyond those of glass are stated by the inventor to be, 1. It is not brittle in any part. 2. Its excitation is more steady, because it requires no amalgam. 3. Its dimensions have no limit.

The power of excitation in this way appears to have been very considerable. The facts are not related with so much detail as could be wished in the Report of the Academy; but it appears that the negative sparks from the conductor of Walckiers, which was five feet long, were from 15 to 17 inches in length, very loud and dense, and very painful to the hand; that pointed bodies emitted very sensible sparks to the conductor; and that a battery of 50 square feet was charged by 30 turns of the machine, which gives 19 feet of silk rubbed to charge one foot of glass *. In another instance, however, it is said, that a square foot was charged by one turn of the machine, which answered to $31\frac{1}{2}$ square feet of silk. It is not said whether the labour of turning was considerable or not.

M. Rouland made several trials to substitute plain silk instead of that which was varnished; and he also tried woollens and mixed cloth containing goat's hair; but none of these answered to his satisfaction.

XII.

Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. Venturi, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c.

(Continued from page 276, vol. ii.)

Proposition VI.

THE expence of fluid is less through cylindrical tubes than through conical tubes, which diverge from the commencement of the contracted vein, and have the same external diameter.

* See Philos. Journ. I. 87.

The

The general theory is the fame for both thefe forms of tubes; but the lofs of living force is greater in the cylinder, and the effect of the communication of motion in thefe tubes cannot approach its maximum as in the cone. Let the tube A C N M, fig. 5. Pl. VIII. of the prefent volume, have the form of the contracted vein in A C F D; the cylindrical part G I N M has its diameter M N, greater than D F. By the reafoning made ufe of in the preceding propofition, it is proved, that the lateral communication of motion tends to produce a vacuum in the folid zone R O Y S X Q T Z. If the communication of motion in this tube were completely made, it would follow, that the preffure of the atmofphere would increafe the velocity of the contracted vein in the ratio of DF^2 to MN^2.

But the form itfelf of the cylindrical pipe always deftroys a notable part of the effect: for the fluid filaments A D, in turning through the curve D R, proceed brifkly to ftrike the fides of the tube G M at R, where they lofe part of their motion. In the fpace D G R eddies, or circular whirls, are produced, as in a bafon which receives water by a channel. Thefe eddies are, to a certain extent, a failure of the effect, and retard the efflux of the ftream. A much lefs increafe of the expenditure takes place in the cylindrical tube than would anfwer to the ratio of DF^2 to MN^2.

Experiment XVIII.—A notion may be formed of thefe internal fhocks and eddies in the cylindric tube, and their effects on the efflux of the fluid, if attention be paid to the following table of the expenditure through the different additional tubes in the horizontal pofition. All thefe tubes have the diameter of their two extremities = 18 lines; they were all provided with the conical tube of the form of the contracted vein at their inner extremity, excepting that of fig. 6. The charge was always 32.5 inches above the centre of the orifice.

Table of the Times employed in difcharging Four Cubic Feet of Water through the different Adjutages.

Through the orifice in a thin plate, - - -	41″.
Through the fimple tube of fig. 6. - - -	31″.
Through the tube of the form of fig. 5. - -	31″.
After having amended (adouci) the conical divergent part, D F I G, of the fame tube, - - - -	30″.
Through the tube fig. 9. - - - -	32″.5
Through the conical tube of the form fig. 10. - -	27″.5
Through the tube fig. 5. the portion G I N M being 23.5 lines in diameter, and 84 in length, the reft as before, -	27″.

It may perhaps be demanded, whether, in the internal part of the fimple cylindric tube K L V of fig. 6. there be the fame augmentation of velocity, and the fame contraction of the ftream, as in the compound tube of fig. 5.? By reafoning according to the principles we have eftablifhed, I think, 1. That in the fection K L of fig. 6. there is the fame increafe of velocity as we have feen (Prop. II.) take place in the fection A C of fig. 5. The direction of the fluid particles which pafs through thefe two fections muft be the fame in both cafes, becaufe this direction can depend only on the impulfe received within the refervoir, which is the fame in both. 2. In fig. 6. the fluid particles, after having paffed through the fection K L, begin immediately to experience the effect of the lateral communication of motion. They muft therefore deviate laterally through the curve L *x z*, before they ar-

 rive

rive at the place of contraction which they assume at D F, fig. 5. and which they likewise assume when the orifice is made in a thin plate. If we imagine a tube of glass *y* K, one extremity of which is applied at K, fig. 6. and the other extremity open in the interior part of the reservoir, it will be seen that the pressure of the atmosphere, which is exerted upon the coloured fluid T, must likewise act on the surface of the reservoir, and join the pressure of the fluid in the reservoir to press the water into the tube *y* K, as it presses the coloured liquor into T S. The pressure of the atmosphere must, in the same manner, augment the impulse of all the fluid particles which arrive at K L, and consequently must increase the expenditure.

Since the checks and eddies in an additional cylindric tube must always destroy a part of the active force of the fluid, it follows, that the fluid column issuing out of the tube can never acquire the whole velocity which is due to the actual charge, and is observed nearly entire in the orifices through a thin plate; and the diminution of velocity corresponds with the increase of the time beyond that indicated by the theory, as may be seen in the following

Experiment XIX. The orifice P fig. 1. being made through a thin plate, and the vertical height P M, being 54 inches, the distance M N of the jet was 81,5 inches. Having applied to the same orifice the cylindrical tube of fig. 5. and the perpendicular P M being let fall from the external orifice of the tube, the distance M N was found to be 69 inches. According to the theory, the expenditure of four cubical feet through this tube ought to have taken place in 26″,24, but it really employed 31″. And the proportions 31″ : 26″,24 = 81,5 : 69 nearly.

The same observation may be made on an experiment of Michelotti (tom. ii. pages 22 and 23). P M being 19,33 feet, and the water issuing through an orifice in a thin plate M N was 23,2 feet; it was no more than 20 when an additional cylindric tube was applied which had not even the proper length.

It is evident, that the theory of the lateral communication of motion must likewise apply in the same manner to descending and ascending tubes, whenever their form admits of this lateral communication. In descending tubes, we must add the increase of expenditure occasioned by this cause to that which is produced by the acceleration of gravity, and which we have estimated in Proposition IV. In ascending tubes, gravity acts in a contrary direction, and consequently its effect must be deducted from that of the lateral communication. Experiment VII. relates to ascending tubes. The following relate to other positions.

Experiment XX. The tube A B F E of fig. 11, Experiment XV. was applied in the place of the tube B C Q O, in fig. 7. The height of the water in the reservoir above the lower extremity of the tube was 41,5 inches. The four cubical feet of water were emitted in 22″.

I applied the same conical tube A B F E, fig. 11, to the orifice R, fig. 8, to form an ascending jet a little inclined from the perpendicular. The height of the water of the reservoir above the upper extremity of the tube was 23 inches. The expenditure of four cubical feet was made in 30″.

The time of the expenditure in Experiment XV. was 25″. And by comparing it with the present, we find nearly $\sqrt{41,5} : \sqrt{32,5} = 25'' : 22''$. And $\sqrt{23} : \sqrt{32,5} = 25'' : 30''$.

Expe-

Experiment XXI. The orifice R, fig. 8, was circular, and 4,5 lines in diameter; the charge was 31,7 inches, and the jet declined a little from the perpendicular. The orifice being through a thin plate, afforded a cubical foot of water in 161″. With an additional cylindrical tube of the same diameter, and ten lines in length, the cubical foot of water was emitted in 121″.

Under a charge of 56 inches, the same orifice afforded, through the vertical jet, a cubical foot in 123″ through the thin plate, and in 91″ with the same additional tube.

These two results being combined, give for the expenditure of vertical jets a mean ratio, between the thin plate and the cylindrical adjutage, of 100 to 134, which is also the ratio between the horizontal jets.

Experiment XXII. I applied the glass tube Q R T, fig. 6, to the point S fig. 5. of the compound tube A C M N, the distance B S being 24 lines. In this situation the fluid T no longer rises in the tube. This proves that the lateral translation of the fluid in the cylindrical tube is made very near the place where the vein is contracted, and that consequently D R must briskly strike the side G M.

By this experiment we see that the distance B R to which the oblique filaments strike the sides of the tube, does not amount to 24 lines. Supposing D O = 20 lines, the time which the particle D employs to pass through the space D O in my experiments is less than 0″,01. Let us decompose the curve-lined motion D R according to the lines D O, O R. Let us suppose the acceleration through O R to be uniform, and it will be found that this acceleration is at least five times as great as that of heavy bodies. If the lateral force through O R were simply the mutual attraction of the particles of the water, this attraction in the particle D must not only overcome the inertia of the particle itself, but likewise that of the other particles nearer the axis, which follow D in its deviation through D R, and impress upon them a much greater sum of acceleration than that of gravity. Now the force of attraction of one particle of water is not greater than the natural gravity of a thread of water of the length of one line at most. The lateral communication of motion, which is the cause of the acceleration through O R, is therefore much greater than could have been produced by the mutual attraction of the particles of water.

Proposition VII. By means of proper adjutages applied to a given cylindric tube, it is possible to increase the expenditure of water through that tube in the proportion of 24 to 10, the charge or height of the reservoir remaining the same.

I shall here give an account of the different precautions necessary to be taken when the expenditure of water through a cylindrical tube of a given length is required to be the greatest possible.

1. The inner extremity of the tube A D (fig. 13.) must be fitted at A B with a conical piece of the form of the contracted vein*; this increases the expenditure as 12,1 to 10. Every other form will afford less. If the diameter at A be too great, the contraction will be made beyond B, and the section of the vein will be smaller than the section of the tube.

2. At the other extremity of the pipe B C apply a truncated conical tube C D, of which let the length be nearly nine times the diameter C, and its external diameter D must be 1,8 C. This additional piece will increase the expenditure as 24 to 12,1. (Experiment XVI.) By this means the quantity of water will be increased by the two adjutages A B C D, in the proportion of 24 to 10.

* Bossut, Art. 509.

At

At Rome, the inhabitants purchase the right of conveying water from the public reservoirs into their houses. The law prohibits them from making the pipe of conveyance larger than the aperture granted them at the reservoir, as far as the distance of fifty feet *. The legislature was therefore aware, that an additional pipe of greater diameter than the orifice would increase the expenditure; but it was not perceived that the law might be equally evaded by applying the conical frustum C D beyond the fifty feet. From this second rule we learn, that it is not proper to make the flues of chimneys too large in the apartments; but that it will be sufficient if they be enlarged at their upper terminations, according to the form C D, fig. 13. This divergency of the upper part will carry off the smoke very well, even when it is not practicable to afford chimneys of sufficient length to the upper apartments. The same observation is applicable to chemical furnaces for strong fire.

3. The pipe B C ought to be straight, without elbows or curvatures. To the experiments which Bossut has made on this head† I shall add the following.

[To be continued.]

ACCOUNTS OF BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1798. Part II. Quarto. 593 pages, with a List of Presents and Index, occupying 12 pages more, and 17 plates. Sold by Elmsly, London.

THIS Part contains the following papers: 10. A Disquisition of the Stability of Ships, by George Atwood, Esq.—11. Quelques Remarques d'Optique, principalement rélatives à la Reflexibilité des Rayons de la Lumiere. Par P. Prevost, Professeur de Philosophie à Geneve, de l'Académie de Berlin, de la Société des Curieux de la Nature, et de la Société Royale d'Edimbourg. Communicated by Sir Charles Blagden, Knt. F. R. S.—12. An Account of the Orifice in the Retina of the Human Eye, discovered by Professor Sœmmering: to which are added, Proofs of this Appearance being extended to the Eyes of other Animals. By Everard Home, Esq. F. R. S.—13. A Description of a very unusual Formation of the Human Heart. By Mr. James Wilson, Surgeon. Communicated by Matthew Baillie, M. D. F. R. S.—14. Account of a singular Instance of Atmospherical Refraction. In a Letter from William Latham, Esq. F. R. S. and A. S. to the Rev. Henry Whitfield, D. D. F. R. S. and A. S. (See Philos. Journal, II. 417.)—15. Account of a Tumour found in the Substance of the Human Placenta. By John Clarke, M. D. Communicated by the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—16. On the Roots of Equations. By James Wood, B. D. Fellow of St. John's College, Cambridge. Communicated by the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.—17. General Theorems, chiefly Porisms, in the higher Geometry. By Henry Brougham, jun. Esq. Communicated by Sir Charles Blagden, Knt. F. R. S.—18. Observations of the Diurnal Variation of the Magnetic Needle in the Island of St. Helena; with a Continuation of the Observations at Fort Marlborough in the Island of Sumatra. By John Macdonald, Esq. In a Letter to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—19. On the Corundum Stone, from Asia. By the Right Hon. Charles Greville, F. R. S.—20. An Enquiry concerning the Chemical Properties that have been attributed to Light. By Benjamin Count of Rumford, F. R. S. M. R. I. A. (See Philos. Journal II. 400.)—

* Fontin. de aquæduct. art. 205. 106 et 112.

† Art. 631 et seq.

21. Expe-

21. Experiments to determine the Denſity of the Earth. By H. Cavendiſh, Eſq. F. R. S. and A. S.—22. An improved Solution of a Problem in Phyſical Aſtronomy; by which ſwiftly converging Series are obtained, which are uſeful in computing the Perturbations of the Motions of the Earth, Mars, and Venus, by their mutual Attraction: To which is added an Appendix, containing an eaſy Method of obtaining the Sums of many ſlowly converging Series which ariſe in taking the Fluents of Binomial Surds, &c. By the Rev. John Hellins, F. R. S. Vicar of Potter's Pury, in Northamptonſhire. In a Letter to the Rev. Nevil Maſkelyne, D. D. F. R. S. and Aſtronomer Royal.—23. Account of a Subſtance found in a Clay-pit; and of the Effect of the Mere of Diſs upon various Subſtances immerſed in it. By Mr. Benjamin Wiſeman, of Diſs, in Norfolk. Communicated by John Frere, Eſq. F. R. S. With an Analyſis of the Water of the ſaid Mere. By Charles Hatchett, Eſq. F. R. S. In a Letter to the Rt. Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—24. A Catalogue of Sanſcrita Manuſcripts, preſented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Eſq. F. R. S.—Preſents received by the Royal Society from November 1797 to June 1798.—Index.

Caſes of the Diabetes Mellitus, with the Reſults of the Trials of certain Acids and other Subſtances in the Cure of the Lues Venerea. By John Rollo, M. D. Surgeon General, Royal Artillery. Second edition, with large additions. Octavo, 628 pages. London, Dilly, 1798.

An account of the firſt edition of this excellent treatiſe was given in the firſt volume of this Journal, page 285. The preſent edition is greatly enlarged; but the time of receiving it, and other circumſtances, prevent me from giving an abſtract of its contents at preſent. The chemical reader will ſee with pleaſure, in one of the articles of the preſent Number, that Mr. Cruickſhank has extended his reſearches into the nature of Sugar and Vegetable Mucilage. See page 406.

L'Art du Blanchement des Toiles, Fils et Cotons de tout Genre, &c. *In Engliſh.* The Art of Bleaching Piece Goods, Thread and Cottons, of every Deſcription; rendered more eaſy and general by means of the Oxygenated Muriatic Acid; with the Method of rendering Painted or Printed Goods perfectly White and Colourleſs. To which are added, the moſt certain Methods of Bleaching Silk and Wool; and the Diſcoveries made by the Author in the Art of Bleaching Paper. Illuſtrated with 9 large Plates in 4to. repreſenting all the Utenſils and different Manipulations of the Bleaching Proceſs: An elementary Work, compoſed for the Uſe of Manufacturers, Bleachers, Dyers, Caliico Printers, and Paper-Makers. By Pajot des Charmes, formerly Inſpector of Manufactures. 8vo. 202 pages. Sold by Dugour and Durand, Rue et Hôtel Serpente, à Paris.

This work appears to be of ſuch great practical utility that I have begun a tranſlation, which will appear in the courſe of the winter, with additional information, which I hope to procure, reſpecting the practice of the intelligent manufacturers of our own country.

Memoires et Obſervations de Chimie de Bertrand Pelletier, Docteur de Medecine, &c.; Or, Memoirs and Obſervations in Chemiſtry, by Bertrand Pelletier, Doctor of Medicine, &c. Collected and arranged by Charles Pelletier, and Sedellot the younger, M.D. &c. 2 vols. octavo, with 5 plates, and a portrait of the Author. Sold at Paris by Croulebois, Fuchs, Barrois, and the principal bookſellers.

The works of Pelletier are known and valued by every ſcientific chemiſt. Moſt of them

have

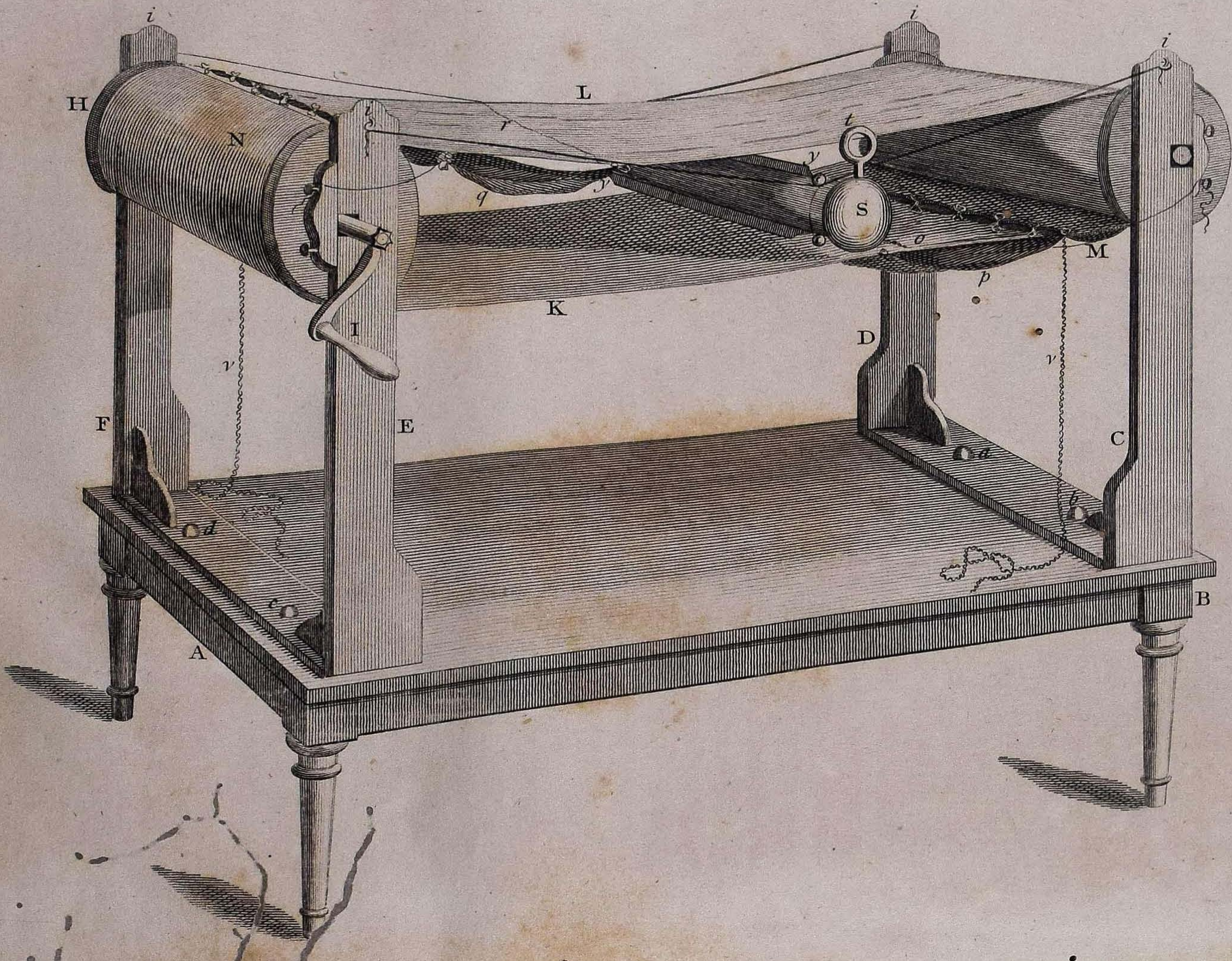
have already appeared in the Annales de Chimie and Journal de Physique; but this collection contains, as Lametherie informs us, some new articles and annotations of the author. The utility of such collections is obvious, even if their sole object were to form into one body the scattered productions of a man of genius. The works of many eminent writers of Memoirs have lost half their utility, for want of this friendly office to science and to posthumous fame.

An English Translation of the late Dr. Stewart's "*Propositiones Geometricæ More Veterum demonstratæ*" is announced for publication by Mr. Leybourn.

It consists of a series of geometrical theorems, mostly new, investigated first by analysis, and afterwards synthetically demonstrated by an inversion of the same analysis. The utility of such a work is evident; and the celebrity of the original, which is now scarce, will render the present translation more valuable.

From a passage in the *Eloge de Leibnitz*, in the volume of the French Academy for 1716, and in the first of the two volumes of *Eloges par Fontenelle*, I conclude that the George Dalgaru, mentioned in page 345, is so named by an error of the press, which but too frequently occurs in copying proper names from obscure manuscript. His name appears to have been *Dalgarme*. I shall give the passage in English, on account of its curiosity and value. (See page 493 of the last-mentioned work.)

"It might now seem as if we had exhausted the subject of the labours of Leibnitz. But "this is not the case; not because we have passed over in silence a very great number of "individual facts, sufficient to have established the fame of any other man, but because "what remains to be narrated is of a very different kind from what we have already given. "It is the project of a Philosophical and Universal Language, which he had conceived. "*Wilkins* Bishop of Chester, and *Dalgarme*, had laboured at this enterprise: but when "Leibnitz was in England, he observed to *Boyle* and *Oldenbourg*, that he did not think those "great men had followed the true method. They might cause nations whose languages "were different, to communicate together with ease; but they had not seized the true "*real characters*, which were the most delicate instrument the human understanding could "avail itself of, and were calculated extremely to facilitate the processes of reason, the "powers of memory, and the invention of things. These characters were to resemble as "much as possible the characters of Algebra, which, in fact, are very simple and very expressive, which never present any redundancy or equivocal expression, and of which all "the varieties are demonstrative. He has somewhere spoken of an *alphabet of human thoughts*, "which he meditated. According to every appearance, this alphabet bore relation to his "universal language. After the discovery of this, it would have been also necessary to have "discovered the art of persuading the different nations to use it; and this would not have "been the smallest difficulty of the task." With regard to this last art, it appears to me to be very simple, and capable of being pointed out in a few words. On this subject the reader may consult the remarks at pages 189 and 191 of our present volume.

Philos. Journal. Vol. II. Pl. XVII facing p.

Powerful Electrical Machine of Walckiers & Rouland.